

R E M A R K S

ON THE

ABSORPTION of CALOMEL

FROM THE

Internal Surface of the Mouth;

Accompanied with

A Preliminary Sketch of the History and
Principal Doctrines of Absorption in
Human Bodies.

In a LETTER to Mr. CLARE,

BY

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Reader in Anatomy.

L O N D O N. MDCCLXXIX.

S I R,

WHEN you shewed me your Essay on the Cure of Abscesses by Caustic, and on your Method of introducing Mercury into the Circulation, you were so obliging as to desire my opinion on the latter of these subjects. I gave you my opinion; and your Essay has now run through a first edition.

That quicksilver, divided by the intervention of hogs-lard, (and forming mercurial ointment), could be introduced into the circulation, from the surface of the skin, or that almost all of the preparations of mercury might be introduced into the circulation, from the internal surface of the intestines, was very well known; but that mercury, in the form of a powder, could find its way into the blood, from the internal surface of the mouth, in sufficient

ficient quantity, to cure the venereal disease, unconnected with any intestinal absorption, was a new doctrine!

I did not expect you would be believed; the doctrine of absorption is still in its infancy; nor have I found that either you, or I, have had sufficient influence to induce belief. The doctrine notwithstanding may be true.

It would be wrong in you to complain of your fate; mankind will not receive new doctrines at first hearing; nor have you any right to expect they should. Inoculation itself met with the most furious and unjust opposition.

My opinion of your method, in the former edition, was stiled *a long Letter*; I have not now made it shorter. Whenever there is the least probability that my opinion may be of use to gentlemen of the profession, or indeed to any serious man, I hope I shall never appear

pear *reserved* or *unnecessarily cautious* in giving it.

The absorbent system has, for several years, *particularly* engaged my attention; nothing, in the smallest degree connected with it, could possibly meet with *indifference* from me.

Common civility entitled you, as a gentleman, to any opinion I could form on your subject; but when I reflected, that you had once been a pupil of Dr. Hunter's, my present situation with him made me feel the *strongest inclination* to do any thing in my power to oblige *his former* as well as *our present pupils*.

When I first sent you my opinion, in writing, concerning the absorption of calomel from the mouth, my remarks were less extensive than those which afterwards appeared in print; for when I understood, that you wished my

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opinion might be made public, I felt that my duty to the public required me to be somewhat more explicit.

Some time has now elapsed since this letter was first offered to the world. I have been censured by some for having too warmly attached myself to one who appears to have been, at that time, an entire stranger to me ; others have not scrupled to say, that I had too hastily recommended your practice.

That you had been discouraged by your medical friends in your important research ; that you yourself appeared diffident in talking of what, you notwithstanding, believed, you had done, added to the reasons already mentioned ; must plead my excuse (if any can be wanted) for my warmth of attachment.

As to the second accusation, I have only to say, that it is not absolutely impossible

possible that we should both be mistaken. I do not yet feel, however, that I have any reason to retract my opinion, or the least cause to repent of my having endeavoured to support the absorption of calomel from the mouth. I seriously declare, that in a matter of so much moment, in which the health of millions *might be* concerned, and where my own reputation *was actually* at stake, no motive could have induced me to recommend a practice I secretly suspected would not be attended with success.

The great hinge on which your method turns, is, the absorbing property of the human body. You affirm that your calomel is absorbed by the lymphatic vessels of the mouth. I find no difficulty in believing that it is; and am satisfied that this, as well as many other powders, may be absorbed by the surfaces of the body.

Before I state the arguments which present themselves to me in favour of

this absorption, I shall take the liberty of making *some preliminary remarks on the absorption of the human body in general*. I have several reasons for doing so.

In the former edition of this Letter, the object of which was the absorption of calomel, I had already, *unavoidably*, thrown out many remarks on this important subject. I did not, on revising these, find that I had changed my opinions ; but as I was at that time much hurried, from my public employment, I could not help wishing that the Letter might be still *more correct* ; and accordingly I have now ventured to withdraw those scattered remarks, interspersed with the arguments for the absorption of calomel, from the place where they stood, and have thrown them together in the form of an introductory sketch of the doctrine of absorption, after which I return to the absorption of mercury. Many of my anatomical friends had wished to see such a sketch of the principal opinions at present held on absorption

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tion at *Windmill-street*: I saw no solid objection either to complying with their request, or to embracing the present opportunity for this purpose.

Such a *sketch* will appear still less unseasonable or unnecessary, if we reflect, that practical books in medicine are now much read by those who, though not educated to medicine, wish to know *something* of their own bodies: when they have been made a little acquainted with *absorption in general*, they will be better able to judge of *your* absorption of calomel.

Every body knows what is meant, when a dry sponge is said to absorb water; it drinks up a certain quantity, and can take up no more till it becomes drier again. The index of the hygrometer (or its spring) absorb moisture from the atmosphere, and occasionally part with it again. In consequence of this, the index points higher or lower
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in the scale, in proportion to the humidity absorbed or given off. Fluids themselves absorb. Vitriolic acid, exposed in a flat dish to the atmosphere, absorbs from it, in the space of a year, six times its own weight of water. All the surfaces and cells of the human body, and most probably of every living body, are also absorbent, and take up, not only *fluids*, but the minutely divided particles of *solids* themselves.

When sponge, dried animal, or vegetable substance, or oil of vitriol, absorb, it is in consequence of properties belonging to *inanimate matter*, in consequence of an attraction which subsists between them and the fluids they absorb. The sponge is porous; its pores, however, could neither prevent nor can accelerate the passage of the absorbed fluid; in short, it is perfectly passive. But the *absorbent pores* of the human body are *the extremities of irritable vessels*, which, in consequence of a property

perty dependent on *life, take up, or reject*, the presented *fluids, or solids*, according to certain circumstances.

In making my remarks on *absorption in general*, I shall suppose myself, obliged to reply to such questions as I conceive would naturally suggest themselves to one, who having never heard of *animal absorption* before, wanted to receive some information on this curious subject. His first question, I presume, would be,

I. *What proofs are there of this absorption?*

THAT human bodies absorb we have now many convincing examples.

Kaau Boerhaave appears to have wrote an express treatise on the following passage of *Hippocrates*, “*δηλον, ἡ αἰσθησις, ὡς ἐκπνοον, καὶ εἰσπνοον, εἶναι, ὅλον τὸ σῶμα*” that is, *it is plain, to any one who*

who attends, that the whole body is endowed with the properties of expiring and inspiring. Hippocrates does not here sufficiently illustrate his meaning. The body inspires when we breath the surrounding air; but this is not what we now mean by *absorption*. It is probable, however, that he really means, as Galen (his best interpreter) and Kaau Boerhaave conceive he does, and that he is to be understood as saying, *the whole body perspires and absorbs*. We do not, however, want the authority, even of Hippocrates, to be satisfied of this circumstance. Boerhaave quotes, in favour of human absorption, the story of *Democritus*, who was reported to have kept himself alive three days, on the *smell of new bread*. Boerhaave does not inform us how long *Democritus* had previously fasted before he begun to smell the bread. If he had not fasted at all before, it does not, in my opinion, prove absorption; but rather, that a man may live three days without tasting any thing; which certainly

tainly may be believed. If he had not tasted any thing for three or four days before, it would look as if something *nutritious*, and which was vapour in the bread, had been absorbed. The Turks, it is true, are said to travel fifty miles a day on a bit of opium held in the mouth; and feel strong and well, merely in consequence of that *excitement* which the opium produces in the brain. Mad men, whose brains are excited from some internal cause, can live long without food; but it would be inferring *too much* to conclude, that the mere *smell* of new bread could produce a similar excitement in the brain, or similar effects on the body.

Paracelsus says, That the antient *Jophi*, least their intellectual powers should be clouded by gross feeding, and their contemplations disturbed, never swallowed any food; that they were kept alive only by chewing it; held it in their mouths sometime, till the finer parts were absorbed, and then spit it out.

He also informs us, that he himself nourished men for several days together, by applying nutritious liquors to the surface of the body. I cannot pretend to say what degree of credit is due to these assertions ; nor respecting the proof for absorption, does it much signify if they should even turn out false.

We have been informed by very antient physicians, that old men have, in some degree, recovered their strength, and lived longer, than they otherwise would have done, in consequence of merely sleeping with young women, and being in their company ; that on the contrary, women so circumstanced, have soon faded ; have lost their beauty, their plumpness, and strength. It has hence been concluded, that the absorbents of the skin in the older person, took up something nutritious from that of the younger one. I have some doubts respecting the fact ; I mean, I do not believe the one became really stronger, and the other weaker. If it could be proved

proved however to be true, I should rather be disposed to account for it in another way. The presence of a young woman may, by pleasing the mind, or warming the imagination, stimulate the heart and arteries to greater action, and rouse them from a torpidity already felt, or which they would otherwise soon fall into. *All Stimuli* are vulgarly thought *unnatural*, and productive of debility ; but there are many *natural stimuli*, which actually strengthen, instead of wearing out the body. The body would languish but for *exercise* ; our *food* itself *stimulates* ; the *most powerful medicines* are *stimulants* ; and the body is generally more healthy when the mind is *much* and *greatly* occupied. On the other hand, the presence of an old man, may commonly, have no pleasant effect on the mind of a young woman, but the reverse, and may prove a constant irritation (or teasing) instead of a healthy stimulus ; and her emaciation may be better accounted for this way. I should also doubt, whether a young child

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sleeping with an elderly nurse lost flesh, from this circumstance. However these supposed facts may be, we have enough of very convincing proofs of absorption without them.

That our *Food*, which is converted into a milky fluid in the stomach and intestines, is absorbed by the *laeteals*, and carried into the blood, *Aselius*, above a hundred and twenty years ago discovered; and repeated dissections of living animals have every day confirmed his discovery.

The furnishing the body with fresh supplies in proportion to the waste of blood, occasioned by the *different secretions*, and the *forming* and *repairing* of parts may appear a sufficient reason why some such process as absorption, should take place, on the inner surface of the intestinal tube. But absorption may be supposed peculiar to this, and unnecessary on other surfaces. Let us see if there are not sufficiently strong proofs, that

that the absorbing power, also resides in every other surface.

Some recent instances of the cure of *hydrocephalus* by mercurial friction, shew that water may be absorbed from the ventricles of the brain.

There are some cases of *hydrothorax* and of *empyema*, in authors of the best authority, where it appears, that water and purulent matter have been absorbed from the cavity of the chest, and that the constitution has, in this way, cured itself.

After the operation for the *Empyema* has been performed, and honey and water, or wine, or bitter decoctions, have afterwards been employed as washes for the diseased surface, there are *some instances* where the injected fluids have been tasted in the mouth.

Physicians have occasionally seen cases of dropy, in which, after every medicine
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had been tried in vain, and the miserable patient had been consigned to his fate, the constitution itself has here also worked a cure, and the whole water of an *ascites*, amounting perhaps to several gallons, has been absorbed from the cavity of the abdomen in a few days, and carried off by stool or urine. Sometimes too, the fluid of *hydrocele*, has been absorbed from the cavity of tunica vaginalis testis, in the dropsy of that part.

That fluids injected into the chest, have been tasted in the mouth; or water in the cavity of the abdomen, has passed off suddenly by urine or by stool; or that *Hydrocele* once formed has disappeared; are no proofs, it may be said, of *absorption*. There may be some common passages between these parts and certain others, which we know are intended as the natural drains of the body; or if there are no such passages as are here supposed, the *linings* of these cavities may

may allow fluids to transfuse. *Anatomy* demonstrates there are no such passages; if there were, what good reason could be given for a fluid's remaining ten months, or ten years, in these cavities, and never taking these passages before. It may be proved, that *transudation*, (or *the soaking of fluids through membranes*), does not take place in the living body, for if it did, it would be impossible that any man, who had water in his chest, should not presently have it in his abdomen; or having *ascites*, should not quickly have the whole cellular membrane of his body also loaded with the water. The liver, or gall bladder, has been found sometimes *gorged* with bile, the most bitter of fluids, without the patient's having once during the disease, a bitter taste in his mouth. The most foetid pus of a *psoas abscess* unopened, or the most putrescent *alvine* secretions, while detained in the intestines, notwithstanding the volatile vapour they appear to contain, never affect

affect the olfactory organs of the body which contains them. If any fluid is more likely to pervade membranes than another, *oil* (or *fat*, which in the heat of the living body is liquid) might be supposed to be that fluid. It has indeed been suspected of oozing through membranes, or the sides of vessels, and lubricating surfaces. The omentum, for example, has been supposed to serve the purpose of furnishing oil, which transuding, lubricated the surfaces of the intestines, and made them glide more easily on one another in their peristaltic motions. The truth, however, is, that oil, in the living body, *never transudes*; it is confined to certain spaces, and never passes its boundaries, which it surely would sometimes do, if it was capable of transuding. There is no oil on the eye-lids, none on the penis, or any where within the scull. The fluid which lubricates surfaces, and gives them their easy play on one another, is manifestly *watery* and not *oily*.

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Though the oil does not *transude*, it may however be *absorbed* from its *cells*. It is accumulated in great quantity, in some habits, when in health, and may be a symptom that the system is acting with vigour; or it may arise from a particular disposition in the arteries, like that of *secreting earth*, and forming *calculi* in other habits; whatever is the reason of its being formed, the oil itself is constantly absorbed when the body is deranged, and under any particular irritation, as in fever, dysentery, dropy, pulmonary consumption, and many other diseases. It is also absorbed in the winter-sleeping animals; who emerge from their lurking places in the spring, notwithstanding their plumpness in the preceding autumn, almost free from every particle of fat. That the fat is absorbed in the human body, under the diseases mentioned, and when exposed to hunger, has given room for ingenious conjectures, founded on the preceding fact, respecting the winter-

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sleeping animals ; and some physiologists have believed, that as in the one instance the fat was absorbed, probably, to be converted into nourishment ; so in the other it was absorbed, and carried into the blood vessels again, in order to nourish the body, either when from want, the stomach was not affording its supplies, or to nourish the body in a less irritating way, than that of throwing a quantity of food into the stomach, in an already diseased state of the machine, as well as of that organ. The jelly found in the stomach of foetuses, seems to support this opinion. I have no idea that the foetus drinks, or absorbs the *Liquor amnii*, which, frequently at the end of gestation, when the foetus stands most in need of nourishment, is in exceeding small quantity, and at other times in a very large and, seemingly, superfluous quantity. It seems more probable that *something* (probably the coagulable lymph) is absorbed by the foetus from the blood of the mother in the placenta ;
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this is afterwards secreted by the arteries of the stomach into its cavity, and converted partly into *chyle*, partly into *meconium*. Ingenious as the idea of absorption of fat in diseases, and in want of food, in order to its *conversion* into *nourishment* may be, and however probable it may appear in the winter-sleeping animals, I have great doubts with respect to its truth in the human body. The body appears to me to be at these times too much under a powerful contrary stimulus, to be capable of the actions of health; such as digestion, and nourishing the body. The stomach receives no food in the leanest person, under the irritation of an inflammatory fever, because it can digest nothing. The only use of employing, in these cases, liquids themselves is, probably that they may be absorbed without being digested; and that materials may be furnished for supplying the different secretions, which go on, and are sometimes even increased not-

withstanding the fever. I have ventured to suppose that watery fluids thrown into the stomach are not digested, because I am much disposed to be of Mr. Hunter's opinion respecting digestion, who thinks that solids, or those fluids, which in the stomach are capable of becoming solids, only, are properly digested, and capable of nourishing the body.

If it is urged, that, in order to nourish the body, the absorbed fat may be only conveyed into the arteries, and by them be immediately applied as nourishment to parts, without supposing it necessary that it should ever be secreted into the stomach, and digested; I have only to say, that it is not probable that any thing else than chyle can be converted into nourishment; and if the fat could be used for this purpose, the arteries are as much in a diseased state, under the present supposition, as the stomach, and equally, as I should think

think, incapable of nourishing the body, as the other is of digestion, or forming chyle.

The mere stimulus of inflammatory fever, or the change which, like opium, it produces in the brain, probably keeps the body alive, and makes any nourishment unnecessary. If fat was absorbed in the human body for the purpose of converting it into food, it would most probably be absorbed for that purpose in the healthy state of the body, as in the natural and sound state of the winter-sleeping animals; and if this was true, a fat man should be able to bear hunger better than a lean man; and it would not be possible that a fat man should die of hunger so long as he kept plump, which we all know is far from being the case.

Thus, absorption takes place, not only in large cavities, but in the *cells* of the *adipose membrane* itself. It also takes place in *those* of the *cellular substance*.

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It has frequently happened that a *rib* has been broke, and its broken ends have entered the substance of the lungs; in consequence of which, the air of inspiration has escaped from the lungs, and got into the cellular membrane of the intercostal muscles, and from thence has passed over the cellular membrane of the greatest part of the body. The modern practice is, to let this air escape by punctures in different parts of the skin. This air has sometimes, however, been left to itself, has dissolved in the fluids of the body, as in common water, and been gradually absorbed.

In the same manner, when a blood vessel, from a fall or bruise, has burst about the neck, and an *internal hæmorrhage* has taken place into the cellular membrane, the skin of the whole trunk of the body has appeared as black as that of an Ethiopian, from the quantity of extravasated blood underneath; the blood, like the air, has been gradually

ally absorbed, and returned into its vessels.

From observations I have made on those who died of the *peritoneal inflammation*, I am convinced, that the absorbents frequently take up the red blood from the *internal surfaces of the arteries themselves*. In this inflammation I have seen parts of the intestines exceedingly inflamed in their peritoneal coat. When I came to examine this inflammation and consequent redness, I found the blood, to which it was owing, was not extravasated; but the greatest part, at least, was really in the cavities of the absorbents, which, on the intestines, are easily distinguished from the arteries and veins, and, on account of the greater absorption intended to take place there, are proportionably larger than any where else. There is in reality little or no swelling of parts in this inflammation, of course little extravasation into the cellular membrane; and, by pressing

pressing the yet fluid blood forwards through the absorbents, I could restore the reddened surface to its original white one. I believe, that *this absorption, from the cavities of arteries*, takes place in consequence of their being too much dilated; for distension of parts appears in many other instances to be one great cause of absorption's taking place in them. When a gall stone sticks in the *ductus communis choledochus*, and prevents the bile from passing into the intestines, the *porii biliarii*, or the excretory ducts, become distended; in consequence of this, absorption takes place from the cavities of these ducts, and *jaundice* is produced. If the milk is not sucked from the nurse's breast, the *tubuli lactiferi* become distended, and the milk is carried off by the absorbents; and from its quantity, or its unusual stimulus, distends and inflames the *axillary glands*. If the bladder is distended with urine, and an opportunity of discharging it cannot be had, it is quickly

quickly absorbed, and returned into the blood; and the bladder, notwithstanding its former distension, on attempting to make water sometime after, is often found almost empty. When the natural cure of *ascites* takes place, it is also, perhaps, in this way. I mean, that the absorbent vessels, which, for a long time before, appear to have lost their irritability, to distension, from a change in the habit, (as sudden as a bar of iron's losing its magnetism from a stroke of a hammer), recover their irritability instantly, and absorb in full vigour.

That absorption takes place *on the surface of the body*, we cannot doubt.

Essential oils, rubbed on the feet, have, in some time after been tasted in the mouth; a proof, not only of their having been absorbed by the vessels of the skin, but that essential oils may pass through the absorbents, and even the secretory vessels, unchanged.

If *expressed oils* are also secreted in the same manner, which I think not altogether impossible, considering how difficult of digestion fat is in many stomachs, and that those who swallow much of it with impunity to the stomach, increase exceedingly in their own fat; if, I say, expressed oil, can be secreted on surfaces, we shall be able to account, in some measure, for the good effects of *oily mixtures* in inflammation of the lungs. I am more disposed, however, to believe, that these effects depend on another principle.

The *juice of the cicuta*, when its fresh leaves have been applied to the abdomen, as a poultice, has also been tasted in the mouth.

Tobacco leaves, beat up with vinegar or brandy, and applied to the pit of the stomach, have in a few hours after produced vomiting. *Groundsel*, beat down into a very coarse pulp, and applied cold to the pit of the stomach, has in the same manner produced the same effect.

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With respect to the effects of *tobacco* and *groundsel*, it may be said, that admitting the cases to be ever so well founded, they do not *prove* absorption. For a part, whose nerves are stimulated by some application, it is well known, may affect a distant part, and produce such an action in that part, as if it itself had been also immediately stimulated by the same application. This consent of parts is by the physiologists termed *Sympathy*. It has been much derided by some philosophers, but is nevertheless a property of living matter, and as good a *term*, and as *intelligible*, in my opinion, as *attraction*. It may be compared with the vibrating of a cord not struck, when it happens to be in *unifone* with one which is struck. No man can say why this takes place; we know it does take place, and are therefore perfectly contented. Ask a musician, Why one cord vibrates when the other is struck? he says, It is because they are in *unifone*. Ask a physiologist, Why one part is

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excited to action, in consequence of a stimulus applied to another distant part, he says, It is because they *sympathize* particularly with one another. Now, certain applications to the skin may, by affecting its nerves, affect those of the stomach, through sympathy, though nothing be absorbed from them. That the tobacco and groundsel required some hours to produce their effect, makes it more probable that their juices were *really* absorbed. When parts are affected from sympathy with other parts, the affection, almost always, takes place *suddenly*. We become *instantly* sick from a bad smell, or a loathsome sight; the rasping of a file *immediately* sets the teeth on edge. The bladder *instantly*, in many habits, sympathises with the ears in listening to the sound produced in emptying a bottle of water, or with the skin in putting the hands into cold water, and gives an almost irresistible inclination to let the urine go.

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But to return to absorption.—The body, we have been assured, has *weighed heavier* after it had remained some time in the warm bath; that it has also *weighed heavier*, during certain humid states of the atmosphere in the morning, than it was the preceding evening on going to rest, notwithstanding the quantity, which, according to *Sanctorius's* experiments, must have been lost by perspiration during the night. Supposing the facts as here stated; we shall be obliged to allow that the increase of weight arose from an absorption of the water of the bath, in the one case, and of the moisture of the atmosphere, or of air itself, in the other.

De Haen, on finding that patients tapped for the dropsy sometimes filled again in a few days, was led to suspect, that such patients *absorb from the atmosphere*.

The passage of some poisons and of infectious matter into the habit, strongly demonstrate

demonstrate *absorption* from the surface of the body ; this we see in the *venereal disease* ; the *poison of the mad dog* ; and the *matter* of the inoculated *small pox*.

The course of the larger absorbent vessels, and the situation of the glands through which they pass, and which are also parts of the absorbent system as we shall see by and by, are now very well known. When the absorbent takes up a poison or matter capable of giving a particular disease, it commonly inflames ; and becoming red, may be traced under the skin, running towards the nearest glands. Having seen the tainted surface, or the wound into which the poison was inserted, it is frequently a very easy matter to be able to foretell where these *red lines* will appear. When the glands have received the poison, they also inflame, swell, and not uncommonly, suppurate. During this period nothing very particular appears in the system in general ; but
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soon after, the marks of the poison, or of the particular infection, become very evident, and shew that it has contaminated the whole body. In this way we discover, not only that a poison, or infectious matter, is entering the body, but the very *road* it has taken.

The trunk of the body and its extremities, as well as the different viscera, have two sets of absorbents, one, which run, on their external surfaces, and another, which is deeper seated. If a poison is absorbed by the first, the *red lines* will generally be evident; if it happens to take the rout of the deeper seated ones, they will not be evident; and even if it is absorbed by the superficial set, unless it stimulates them very much, it will not constantly inflame them; of course they will not be *red*, will not be evident.

Let the infected fore however be any where about the *head*, and if there are
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not *red lines* leading from it towards the sides of the neck, *the glands of the neck*, at least, nearest the fore, will almost certainly be inflamed, and will swell.

Let the fore or wound be any where in the *arms*, or about the *breasts*, the *red lines* will be seen running towards the *axilla*, if they are very superficial; at any rate the *glands there* will be inflamed and swell. If the poison or infection is deposited in any part of the *lower extremities*, or about the *parts of generation*, the *red lines* will appear running towards the *groins*, and the *glands there* will be inflamed and swell. Two circumstances still further confirm this doctrine. *The one* is, that if the gland is cut out while the poison is recently lodged in it, the disease will *frequently* be prevented. *The other* is, the success attending the common method of curing the venereal disease, by rubbing mercurial ointment

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on and around the infected surface, or so as to send the antidote through the same vessels which took up the poison.

I said that the absorbents also took up *solids*.

From *Du Hamel's* experiments, in feeding animals with *madder*, it appears,

That such parts of the cartilages as were then ossifying, became *red*;

That, though all the *growing bones* became *red*, those which were intended to be the *hardest*, became *reddest*.

This last circumstance makes it more than probable, that as bones derive their hardness from earthy particles uniting with the animal part, the *colouring part of madder* is a fine *earthy powder*; that this powder is at first absorbed from the intestines by the lacteals, and afterwards deposited in the bones by the arteries. From the same experiments

it appears, that on leaving off the use of the *madder*, the bones of those birds which had been fed with it, and which hitherto appeared as red as scarlet, even through the living skin, in a few months became perfectly *white* again. From this circumstance we must infer that the *earth of bones* is absorbed, and that the bones themselves are perpetually changing.

If that part of the *urinary calculus*, which happened at the time to be forming in the bladders of some animals, subjected to similar experiments, became red, in proportion as the bones became whiter, we have still an additional proof, that the colouring part of madder is earth, and that *solids* are absorbed.

That the earth is absorbed from the bones, (and the animal part probably also), cannot be doubted, when we know that the thigh bone, for example, of a man at seventy, though little different
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in size, is three times lighter, is weaker and more spongy than it was at thirty, or than that of a young man: it certainly was once as heavy as the young man's; what is now become of the matter it formerly contained?

If the *calculus* became red as the bones became white, it becomes more than probable, that the earth, which is absorbed from the bones, is that which is afterwards employed to form the *calculus*.

Physiologists have hitherto supposed, that when any of the softer or harder solids of the body were removed by a *disease*, that it was in consequence of a *dissolving menstruum*, furnished for that purpose by the constitution. Thus *pus* and *blood itself*, have been accused of dissolving, not only *soft parts*, but the *bones* themselves. The idea was not altogether *unphysiological*, since a very eminent modern *physiologist* allows, that our food is dissolved by a *menstruum* fur-

nished by the stomach ; and that after death, this *menstruum* can dissolve the stomach itself. From a variety of circumstances, and comparing all the appearances together, Mr. *Hunter* is persuaded, that the processes of *digestion* and *ulceration*, (or *the removing of solids*), in no one circumstance are like one another ; that in the last there is no *menstruum* ; and that the *absorbent vessels* of animal bodies are, the only *counteractors* of the arteries ; and alone concerned in removing parts ; nor does he find it more difficult to conceive, that an absorbent, in removing a bone, should take up a particle of earth, than that an artery should deposite it, in forming a bone.

The *alveolar processes*, as soon as the teeth have dropt out, are absorbed in old men, the cavity of the mouth becomes less ; hence the approximation of the chin to the nose, the superfluity of lips, and uncomfortable redundancy of

of tongue, observable in most old men. There was *no pus, no blood*, nor any *menstruum*, which can be proved, which could dissolve so large a portion of bone as an inch depth of each jaw, through its whole circumference.

The *wasting* of the fangs of a *long neglected dead tooth*, some months after it had been inserted into a *living jaw*, in what is called the *transplanting of teeth*, seems to invalidate somewhat the former theory. So does the *wasting of the central part* of what is called a *dead bone*, after every living process had ceased in its circumference; after it had become loose, and was totally unconnected with the surrounding living bone. This sometimes has happened in the *exfoliation*, for example, of nearly the whole of the *parietal bone*. As to the appearance of the tooth, I own I am unequal to the task of solving it. Mr. *Hunter* believes that the living vessels not only absorb *living animal solid*, but in certain circumstances

cumstances crowd on and absorb the *dead solid*.

I have suspected, however, that in the other instance, though the greater part of the bone might be deprived of life, some parts of it might still continue to live. For bones do not receive their vessels from one *centre of ramification*; there is commonly one or two large *ones*, but there are innumerable lesser *ones*. The vessels of the *periosteum* communicate perpetually with those of the soft parts above it; in which case, supposing periosteum to be pretty generally detached by the accident which in part killed the bone, some portions of it may adhere, and be able, not merely to support the life of certain parts of the bone under them; but may be equal to the active process of *ulceration* and removal of parts in the bone. If *pus* was capable of dissolving dead bone in general, it will be very difficult to give a reason why it should
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fix on certain spots, and leave others untouched, when it lay equally over the whole surface.

II. Having endeavoured to collect the proofs of human absorption, I shall next suppose that I am required to give some account of the vessels which perform this absorption. I have already mentioned the absorbents and their glands, but in a general way. Physiologists themselves might not be certain what particular vessels I meant, for many have believed, and some still believe, that the common veins only absorb; or that if lacteals and lymphatics also absorb, the office is divided equally between them and the veins.

Before the discovery of the lacteals by Aselius, certain extremities of the red veins were supposed to be the only absorbents. The veins, as was believed, had two origins; one, a continuation of the same tubes, of which the arteries were

were formed, but *reflected*; by *this*, the arterial blood was returned to the heart. The *other*, an assemblage of small branches, which arising, by open mouths, from surfaces and cells, and soon joining those which were continued from arteries, served to absorb the *chyle* and other fluids. After *this anatomist* had discovered that the *veins* did not absorb the *chyle*, still they were imagined to absorb on every other surface except that of the intestinal tube; and that absorption was performed there by an *uncommon set of vessels*, was supposed something peculiar to that surface.

The *lymphatics*, (or the *lacteals* of other parts), were next discovered by *Rudbeck*, twenty or thirty years after the *intestinal lymphatics* (for they are the same kind of vessel) had been discovered; he appears to have entertained an idea, that *they* also, some how, *absorbed* fluids. Speaking of his new vessels, he says, “*intus, excavata & fistulosa*

tulosa sunt, infinitas habentia valvulas, semper vesiculam chylosam, sive ejus ductus, adspicientes, ne humor a glandulis, vel aliis partibus, exsuctus, iterum refluat." And speaking of this *humor*, afterwards, it appears he meant, *interstitial fluid*, or the fluid of surfaces, for he calls it *humor calore exsudatus*. Glisson also appears to have had similar ideas; and as Haller says, "*a venis, hanc resorptionem, ad vasa lymphatica, transferre tentavit.*"

I cannot admit that Willis's conjecture, concerning the use of the lymphatic vessels, conveys the *idea of absorption*. He expressly says, the fluids come into the lymphatics in form of *vapour*, and are there condensed into *lymph*, "*vapores a sanguine accenso, emanentes,*"——"*in aquam per hæc vasa appropriata—condensantur.*" And still keeping up the idea of *distillation* rather than *absorption*, he bye and bye says of the same vapours, "*condensatas, per lymphæductus,*

phæductus, quasi per totidem alembici rostra, extillent." Bartboline appears to have believed, that these *lymphatic vessels*, like the red veins, were continued from the extremities of the arteries, and that when the arteries had carried the rich blood to different parts, for nourishment or secretion, these vessels carried back the lymph which had been employed as *a vehicle* for the nutrient part of the blood. The greater number of *physiologists* seem to have acquiesced in this opinion; and though they allowed that the *lacteals* absorbed a fluid, denied a similar office to the *lymphatics*. Dr. Hunter, from comparing the *lacteals* and *lymphatics* together, observing that their structure and appearance were the same; that neither of them were *easily* injected from the blood vessels; and particularly from observing, that *poisons* or *infectious matter* in entering the blood, always took the course of the *lymphatic vessels* and their *glands*, taught, that *lymphatics* and *lacteals*, were the same kind of vessels; that

that *their common office was absorption*; and that *this office was not divided between them and the red veins, but belonged to the lacteals and lymphatics only.*

Baron Haller, one of the greatest physiologists we have had, though he admitted absorption by *lacteals* and *lymphatics*, contended, however, that the *red veins, also* absorbed. Other eminent anatomists were also of his opinion:—

“*respondebimus interim (says he) multo amplius, resorptionem patere, quam venarum lymphaticarum imperium; & ibi peragi, ubi nunquam, certa fide, ejusmodi vasa ostensa sunt, ut in cerebro, pleura, peritoneo, & cute.*” He not only be-

lieved that *lymphatics* did not exist in many places of the body, but he thought he proved, from his injections of the *veins* in the dead body, that *they certainly opened at their extremities on surfaces*; and as they thus allowed the injection to escape on these surfaces, so they might also, by the same ori-

fices, absorb from these surfaces: “*neque raro vidi (says he) cæruleam ichthyocollam, quæ de venis, exhalaverat, pericardii figuram expressisse.*”— “*Et iterum, figuram ventriculorum cerebri, glutine piscium, per venas impulso, non semel conservatam vidi; ut manifestum sit, a venis, in eas omnes caveas, liberum iter esse.* Haller’s authority is, beyond controversy, respectable; but his injections were thrown into *dead bodies*, which, we know, allow fluids to *transude*. Mr. Hunter, maintaining the same opinions as his *brother*, with a view to decide this curious point, made a variety of experiments on living animals; and proved, in my opinion, that the *red veins* do not absorb on the *intestines*, and made it therefore more than probable, that they absorb *no where else*. On no occasion did he find, that the *veins* took up *chyle*, coloured fluids, or *solution of musk*, purposely thrown into the cavity of the intestines; nor, on the other hand, when he injected the
mesenteric

mesenteric veins, did he find, that any thing escaped from their extremities into the cavity of the intestines.

I cannot help saying, that when *Haller* informs us, that *there are no lymphatics in the brain, because they have not been seen, and that red veins must therefore absorb in the brain, he infers too much.* *Haller* and *Senac* have informed us, that they never could discover *lymphatics* on the *human heart*; where I have been fortunate enough to inject *some thousands.* The *brain* becomes soon, after death, so soft, that it will not bear injection. In the young animal, it seems little else than a *thick fluid.* One intention of this, perhaps, is, that it may better bear *compression in the birth,* than it would have done had it been more *solid.* Maceration in water is the method by which the absorbents of parts may generally be detected; but if the *brain* is, of itself, so tender as not to be handled, or spoils
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so quickly as not to bear maceration for a sufficient length of time, and if *lymphatics* are with difficulty discovered in very firm parts, no wonder we have been so unsuccessful in attempting to find the *lymphatics* of the *brain*. I have seen *absorbent glands* in the *foramen caroticum* in the basis of the scull: they could not possibly be there, unless there were absorbent vessels, to which they belonged, also passing that way, and coming from *within the scull*. If we discover absorbents in the right leg, we may surely infer, from the known uniformity of Nature in her works, that they also exist in the left, though we have not seen them. The office of the *veins* is, *to return the blood from the arteries to the heart*; the office of the *lacteals* and *lymphatics* is evidently *absorption*. Why should we suppose, that when one set of vessels are expressly formed for absorption, a double task should be imposed on the *veins*, whilst, at the same time, the numbers of the

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confessedly absorbing vessels demonstrate that they must perform some great and important office in the machine?

III. Having set out with the idea, that the *human absorbents* took up *mercury* in the form of a fine powder, some account of *their orifices*, will also be expected. This unavoidably leads me into a very curious and extensive field. Almost all the *anatomists*, hitherto, have been persuaded, that *these orifices* were too minute to be visible even in the microscope; that *this minuteness of their orifices* answered a very good purpose, and served as a guard on the constitution, to prevent any thing *coarse* from entering the blood. Whoever sets out in search of these orifices, will most naturally be led to the *internal surface of the intestines*. He must perceive, that his probability of success will be greater on that surface than any where else in the body. The different vessels are there elongated into *processes*, which,

which, to the naked eye, seem, like hairs, or the pile of velvet, to project from this surface, and are therefore termed *Villi* by the anatomists. Each *villus* has its little *artery*, *vein*, and *absorbent*; though we cannot demonstrate, *anatomically*, yet the *colic* convinces us sufficiently, that they also have *nerves*. These vessels and nerves are connected by cellular membrane, and have also a kind of *cuticular covering*.

Haller says, this *cuticular covering* is easily demonstrated in the great intestines. I have seen it on the small intestines. I own I doubted of the truth of their doctrine who asserted, that *cuticle* was continued from the *mouth* to the *anus*. I have seen the cuticular lining of the *æsofagus* evidently terminate within the *cardia*, by a regular border. I have seen *this* in the human subject. It has been long ago observed in the *ass*. There is an appearance which might lead one to imagine, that it even terminated in some animals

animals in that part of the *œsophagus* which is immediately below or opposite to the *larynx* as in the *cat*. This last instance is certainly a *deception*. It is a *particular fold*, but no ways resembling the *cuticular* termination in the *cardia* of the *ass*. I will not say, that the *cuticle* does not terminate a little within the *stomach* in some animals. But I am convinced, that the *rete mucosum*, one *lamina* of which I can demonstrate to be in every respect like the *cuticle*, is continued over the intestinal tube, and covers the *villi*. *Rete mucosum* seems to be wanting in the *sole of the foot*, and to terminate soon within the *lips* and *nose* in the *Negroe*. I know it does not *terminate* there, but only *changes its colour*. The constituent parts of the *villus* are not surely connected and covered by *cellular membrane only*. If this were the case, the *fluids* and *flatus* of the intestines would, from their *peristaltic motions*, be forced into this *cellular membrane*, and pervade the whole body.

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In the *dog*, the comparing the *villi* to *bairs*, is not altogether improper; but viewed in the microscope in *men*, they resemble more a *transverse section* of the end of one's finger, were the finger not quite so round, but flatter before and behind, of course not so thick on the sides. This section of the finger must also be supposed diminished, almost to the size of a pin's head, the *villi* will then have two broad sides, and two narrow ones; one loose floating oval edge, and one straight fixed under edge. Each villus is a kind of *valvula connivens*, in miniature, and, like it, intended to increase the surface.

I had learned, that *Leiberkuhn* had seen the *orifices of the lacteals* on the tops of these *villi*. Mr. *Hewson* at one time informed us, that he also had discovered the orifices of the lacteals on the intestines of the *goose*, and that *they began by a pair of valves*. He appears to have deserted this opinion,
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in his last publication; and having seen *something* of the appearance *Leiberkuhn* describes, talks of the absorbent orifices in a very vague manner. In one page he says, “ I think I can clearly shew the orifices of the *lacteals* ;” in the next, “ the extremity of the *vil-lus* seemed spongy and porous ;” and bye and bye he says, “ Having, on repeatedly examining them, observed the pores, or orifices, very distinct and empty.”

I was satisfied with neither authority. — Tho’ I had frequently looked for them, I never saw any thing like *the orifices of lacteals*. At last, about a year and a half ago, a very favourable opportunity of investigating *these orifices* presented itself. A woman had died *suddenly*, about four in the morning, after having been in pretty good health on the preceding evening. Her relations wished to know the cause of so sudden death; and the body was opened. The

lacteals, on the outside of the intestines, and along the *mesentery*, were more turgid, with a *firm coagulated chyle*, than I had ever seen them. They were also, in proportion, more numerous, and gave an idea of their being, at least, equal in number to either *arteries* or *veins*. The *mesenteric glands*, instead of putting on their usual *reddish* appearance, were, from the quantity of *chyle* they contained, *perfectly white*. When the intestine was opened, there appeared a number of *white points* up and down its internal surface; in some places they were crowded together, but in general scattered. These *points*, on closer inspection, would have made one suppose that each *villus* was a *vesicle* turgid with *chyle*. The arteries and veins were so compressed from the distension of the extremity of the *lacteal* by the *chyle*, that the whole *villus* seemed to belong to the *lacteal*. Though other *villi* were not equally white, or distended, I afterwards found
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they all contained *chyle*. In a portion of this intestine, subjected to the *microscope* in a strong light, I saw distinctly a number of *pores* on the tops and sides of those villi; I was convinced they were the *absorbent orifices of the lacteals*. They appeared to be in a kind of *bulbous extremities* of those vessels, and their diameters, as I had often supposed, *a priori*, were, in reality, several times larger than those of the *particles of the blood*, seen under the *same microscope*. I had a drawing made of some of the more distinct turgid villi, and of *these absorbent orifices*. Dr. Hunter, Dr. Jebb, and several of my anatomical friends, were occasionally present, saw these appearances, appeared to be satisfied; nor can I suppose that I was deceived. The reason, probably, of my success here, was, that the *chyle* coagulates in the dead body; and gives that *erection or distension* to the villi which they had, when under the *stimulus* of absorbing. Without this they collapse,

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particularly in the dead body, and the *orifices* become invifible.

I next fet about discovering *theſe orifices* in the *ſkin*, but with little hopes of ſucceſs. The extremities of the veſſels do not *there*, as in the inteſtines, form *villi*; except on particular parts, as on the *lips, tips of the fingers and toes, palms of the hand, or ſoles of the feet*. Or if the veſſels do form *villi* in other parts of the *ſkin*, theſe are ſo ſhort, ſmall, and crowded, as to make the ſurface ſeem ſmooth. Where the *villi* are long they are ſtill ſmall, compared with thoſe of the inteſtines. The *lymph* is never, perhaps, abſorbed with ſuch velocity, or in ſuch quantity, in a given time, as the *chyle*; or ſo as to make the *villus* of the ſkin as turgid as the *inteſtinal one*. But ſuppoſing it is, we cannot determine the time of this *turgeſcence* as in the inteſtines; and if we could, the *lymph* is ſtill transparent, and therefore can never give that diſtinctneſs, which
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a white coagulated fluid produces, in the *villi* of the intestines. *Leiberkuhn*, we are informed, in order that he might be able to find the *orifices of the lacteals*, gave his patients *milk* to drink before death, and found it afterwards coagulated in the *villi*; but had he bathed the *hands* and *feet* in milk before death, I doubt if the *absorbents of the extremities* would *then* have taken it up, or if they had, and had taken it also in considerable quantity, still I believe it would not have coagulated, nor made the *villus* turgid, as it had not passed through these previous changes in the stomach and intestines, which convert it into *chyle* and dispose it to coagulate. The intestines appear *to live* after the other parts of the body, *are dead*, and absorption frequently takes place from their internal surfaces, after the animal has *expired*; but there are no facts, which tend to prove absorption in the *extremities* in the recently dead body; they are parts which generally *die* first. The
ceasing

ceasing of the *pulse* in the *wrist* before it ceases in the *middle of the arm*, proves this, and shews that the *blood* is retiring to the *internal parts*; whilst the *sub-sultus tendinum* equally shews, that the *brain* is also beginning to withdraw its *stimulus*.

Though I did not succeed in finding the *absorbent orifices* on the *villi* of the *skin*, I had formerly injected the *absorbents of the skin* with *quicksilver*: I had not the least doubt of their being there. I was also led to make several observations on the *pores of the skin*, on the *cuticle* and *rete mucosum*, which, as connected with the manner in which I suppose *absorption* begins on that surface, may not be *impertinent* here.

Remarks on the Skin, and its pores.

WHEN a *blister* has been applied to the *skin of a negroe*, if it has not been very stimulating in twelve hours after, a *thin transparent*

transparent greyish membrane is raised, under which we find a fluid. This membrane is the *cuticle* or *scarf skin*. When this, with the fluid, is removed, the surface which was under them appears *black*; but if the *blister* had been very stimulating, *another membrane*, in which this black colour resides, would also have been raised with the cuticle, this is *rete mucosum*, which is itself double, consisting of *another grey transparent membrane*, and of a *black web*, very much resembling the *nigrum pigmentum* of the eye. When *this membrane* is removed, the surface of the true skin, (*as has hitherto been believed*,) comes in view, and is *white*, like that of a *European*. The *rete mucosum* gives the colour to the skin; is *black* in the *Negro*; *white*, *brown*, or *yellowish*, in the *European*. The reason why this membrane is *black* in the *Negro*, is, perhaps, that his body may be better able to defend itself against *the sun's rays*, and that the *heat* may be prevented from penetrating. The

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intention of a similar membrane behind the *retina* in the eye, appears to be, not only that of *absorbing* the superfluous rays of light; but according to the *ingenious* optician, Mr. Storrer, like the *amalgam* behind the *looking-glass*, of enabling the *retina* to *reflect the rays* in order to perfect vision. It is not very improbable that some such purpose as enabling the *cuticle* to reflect the sun's rays in those warm climates, where the inhabitants originally go naked, may be the intention of nature, in giving them the *black membrane*. Perhaps too, the circumstance of the countenance's becoming *brown*, when exposed to the sun's rays in summer, in our own climate, may be a process of nature to defend herself against the access of external heat into the body.

Both *cuticle* and *rete mucosum* send innumerable *processes* into the *pores* of the true skin; the *process* of the *rete mucosum* is always within that of the *cuticle*, and in contact with the sides of the

the *pore*, as formed by the true skin. These *processes* are remarkable in the *cuticle* and *rete mucosum* of the *elephant*, and some of them are almost an inch long; the *cuticle*, or *rete mucosum*, or a membrane very similar, and having the same properties with these, appears to me to be also continued into the inside of the *mouth* over the *tongue*, *internal surface of the lungs*, *œsophagus*, *stomach*, and *intestinal tube*. In most of the last named parts, the *cuticle*, however, forms *sheaths* for *villi*, and not *processes* which line pores. On viewing the surface of the skin, even with the naked eye, we find it *porous*; more so in some places than in others; and that the *pores* are also larger in some parts than others. These *pores* are either *ducts* of *sebaceous glands*; serve to transmit hairs; (and in my opinion,) the greatest part of the *perspirable matter* itself. Absorption on the skin also, in all probability, begins on the sides of these pores. They are particularly remarkable about the *mouth*, *nose*, *palms of*

the hand, soles of the feet, on the external ear, scalp, mons veneris, and around the nipple in women. Grew thinks he was the first who observed them on the fingers; and has given a pretty just engraving of them, in the *Philosophical Transactions*. Winslow describes these last, and says, they are the *ducts of glands*. The processes which line the pores transmitting hairs have been long observed, but I do not recollect that any *anatomist* has described these processes which line the other classes of pores. *Albinus* takes notice of the appearance, but says, (if I am not mistaken), that they are the *roots of hairs* pulled away with the *cuticle* or *rete mucosum*. The processes which line the pores, would however, from what I can collect of the opinions of the most eminent latter anatomists, be reckoned *imperforated*, and described as so many *blind pouches*, resembling the fingers of a glove, which might be pulled out of the pores entire, by long maceration of the skin in water. Of course the *cuticle*
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and *rete mucosum* would, in their opinion, be reckoned *every where entire*; and it must be owned, that when these *membranes* are separated by *maceration*, and viewed in the *microscope*, there is not the least appearance of *pores*. *Haller*, *Albinus*, and *Meckel*, are of opinion, there are no pores in these membranes. None of the latter *anatomists* have been able to discover *Lewenhoeck's scales*, of which he believed the *cuticle* was composed, and between whose *interstices*, or *loose edges*, the fluids passed into the body, or passed out. *Malpighi's* and *Ruysche's perforations* of the *rete mucosum* have been sought for with no better success. I own, that after some pains, and assisted by pretty good *microscopes*, I have not been able to discover *perforations* in the *cuticle* or *rete mucosum*. It is true, that by macerating the *tongue of a calf* in water for a considerable time, an appearance of pores may be produced in the *rete mucosum*; and it is as true that the same appearance may be

be produced in the *cuticle*. But when the *one* appears *perforated*, the corresponding surface in the *other* is always not so; and where the *processes* are short, and easily separate from one another, neither *cuticle* nor *rete mucosum* appear to be *perforated*; and both may be demonstrated to have their own *processes*. *Malpighi* first taught that the *rete mucosum* was *porous*. I find *Haller* of the same opinion; also asserting from *Du Hamel*, that the *rete mucosum* in the *feet* of many *birds* (he particularly instances the *Ostridge*) is *perforated*. I have seen those talked of *perforations*, and am convinced, that as in the *tongue of the calf*, these are only *vagintulæ*, or sheaths for the *villi*, and cannot be demonstrated by any means to be open at top.

Though I have not found *pores* in either *cuticle* or *rete mucosum*, I believe nevertheless, that *they* certainly exist, and for reasons which I shall give presently; but

but as I never could see *them*, I think it justice to those gentlemen who admit nothing which is not *demonstrable*, to say so. *Albinus* and *Meckel*, particularly the *last*, are disposed to believe, that whatever fluids are *perspired*, or whatever are *absorbed* by the skin, must equally *soak* through the *cuticle*, as the vapour of warm water does through dried leather. *Albinus* even doubts, whether the *perspired fluids* do not ooze through the *coats* of the extreme arteries themselves, as vapour, and are afterwards condensed into *sweat*. “*Quid nī* (says he) *penetraret, per mollia nostra, humidaque, quum calentis aquæ vapor, per durum, siccumque corium, eo modo penetraret?*” *Professor Meckel* uses nearly the same language. Talking of the *cuticle* in the *memoirs of the Academy of Berlin*, he says, “*Quoiqu’ inaccessible aux vaisseaux, sa nature est pourtant telle, qu’il transmet le liquide, dont il est imbu, a peu près, comme pourroit le faire un cuir mince humecté.*” He also observes, that though

in the palms of the hands of *blacksmiths*, and in the soles of the feet in *travellers*, the *cuticle* consists of many layers, and is sometimes a quarter of an inch thick, still perspiration takes place on these surfaces. Did the *fine perspiring vessels* reach the *cuticle* of the *foot* in the one instance, or of the *hand* in the other, the *weight* of the body, or the recoil of the hammer, he thinks must crush them to pieces.

Notwithstanding of such *respectable opposition*, I cannot help being persuaded, that such a process as *soaking*, however it may take place in dead animal substance, or vegetable, is a process too much allied to those of *dead matter* to have any place in a *living body*. Nay, I think it may be proved, it never does take place in *cuticle*, even in the dead body. There are difficulties, however, on both sides. Let us examine the different facts.

The reasons which induce me to believe that there are *pores organized*, connected with the extremities of the *exhalent arteries*, in the *cuticle* and *rete mucosum*, which, however invisible in the *dead separated cuticle*, still exist, and are sufficiently dilated in the *erected state* of the extremities of the vessels of the *living and perspiring skin*, are the following :

When a piece of *cuticle* falls off from the *cutis*, some of the *hairs* go with it, and some remain with the *cutis*. Those *hairs* certainly *perforated* the *cuticle*, yet, in the *microscope*, not the least vestige of these *perforations* can be traced. In places where the *hairs* either do not exist, or where they are invisible, *where*, however, the *pores* are very numerous, as on the *nose* and some parts of the *external ear*; no *perforations* can be traced in the *separated cuticle*; though the *sebaceous matter* could formerly be pressed from the cavities of these

pores on the *nose*, in form of a small worm, of some considerable length. The *processes* themselves are frequently *tore off*, and remain with the pores of the *cutis*, yet no appearance of *perforation* is seen in the separated *cuticle* of any such part of the skin. I perforated pieces of *cuticle* with a *fine needle*, but these *perforations* were *invisible* in the *microscope*, as they would have been had I perforated the *elastic gum*. The pores of *filtrating paper*, when dry, are very manifest in the *microscope*; but on wetting this paper, they become invisible. The *dead cuticle*, and even the *callous living cuticle*, swell from water, though the sound parts of *living cuticle* do not seem to undergo any change from lying long in water. The *cuticle* of the *palms of the hand*, and of the *soles of the feet*, seem at least to imbibe moisture; but the *cuticle* on the *opposite sides* of the hands and feet do not appear to have undergone any change. If *dead cuticle* swells in water, its *pores* will inevitably become

become invifible. I fhall, bye and bye, offer fome reasons, for making it probable at leaft, that the *first perfpiring* and *abforbing pores* are in the *proceffes* or *vaginulæ* of the *cuticle* and *rete mucosum*, and that thofe which appear on the outside furface are *secondary*, refemble *mucous ducts*, and are common to a vaft number of the *primary pores*. Farther refpecting the *foaking of fluids* through *cuticle* and *rete mucosum*, let it be remembered, that in many *fevers* the fkin is for a long time *parched* and *dry*, though it *looks red* and *feels hot*; the laft circumftances prove, that the *blood* is derived to the fkin in greater quantity than at other times, yet the fluids do not *sweat* out, and much lefs *transude*. Many people, notwithstanding their uſing exerciſe, even in hot weather, when the fluids muſt be determined to the fkin, do not ſweat. I have ſeen *veſications* take place from *burns*, from other accidents, or from the conſtitu-

tion ; these have been left to themselves ; the fluid has not appeared sensibly to evaporate ; they have remained, apparently, of the same size, for eight or ten days, without the *cuticles* ever feeling *moist*. When a bit of skin, with its *cuticle* sound, and adhering, is exposed to air, it will be many weeks in *drying* ; and were not the cuticle to separate by *putrefaction*, would probably never dry at all. I exposed such skin to the heat of 100° or 120° for two days, without its appearing to have dried in the least. When *cuticle* happens to be rubbed off, the skin dries *immediately*. Though the legs in *œdema* are loaded frequently with *lymph*, not a drop *transudes* through the cuticle, unless the distension has been so great as to tear it, which rarely happens. Is it probable that the *same cuticle* should be the most *permeable* and the most *impermeable*, to fluids of any substance, at one and the same time ?

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You yourself, it may be said, allow of pores ; why, if these pores exist, does not the fluid of *vesication* escape by the pores, though it may not transfuse ? These pores, I have already said, I believed were in the *processes of cuticle and rete mucosum, which lined the pores of the skin*. If one presses his finger about the middle in hot weather, or applies a ligature, the *perspirable matter* will be forced out at the pores on the tops of the fingers, in *round drops*, at regular distances, on the *spiral ridges*, like the secretion of the *tarsal glands* of the *eyelids*, after they have been immersed in spirits. In the *latter case*, the equal pressure of the surrounding fluid may oblige the secretion to put on the appearance of *round drops*. But I will not admit *Albinus's* reasoning as just, when he says, the fluid perspires, in the *former instance*, from every part of the skin, and is collected into *drops* by the equable pressure of the surrounding *atmosphere*. I see the drops appear at the
orifices

orifices of the pores, and no where else; and their rounded form, depends on their being accumulated in a round cavity, the *orifice of the secondary pore*. This makes it more than probable, that the *perspiring pores*, and from analogy, the *absorbing pores*, are in the *processes* of the *cuticle* and *rete mucosum*, which line the *secondary pores* of the *cutis*, and not in that *apparent* external interstitial surface of the cuticle itself, placed between the mouths of the external pores. What further confirms this idea is, that the parts most porous *sweat* most, and, I dare say, will be found to *absorb* most. The *tip of the nose*, in warm weather, the *head*, the *arm-pits*, the *soles of the feet*, and *palms of the hands*, sweat most. Now, though I contend that there are pores in the *cuticle* and *rete mucosum*, still I think it possible to give a reason why the cuticle does not allow the fluid of *vesication* to escape.—When cuticle is detached by *vesication*, its processes must be compressed against its

internal

internal surface, and the pores of course will be shut. When *œdema* distends a limb, the fluids do not escape for another reason. The extreme arteries, which exhale on the skin, are, probably, compressed by the water, and the cellular membrane become turgid, by exhalation, from more internal branches : and besides, *sweating* is a secretion which I cannot conceive consistent with the distended state of the *cold* skin ; we have therefore no moisture, in general, from such surfaces. I have said, that though I exposed skin to heat, sufficient to convert its fluids into vapour, (and as vapour is allowed to be more penetrating than fluid, it should of course have dried quickly), that it did not. Now, if the *villi* are either supposed to be *collapsed* or the *processes compressed*, one may see *some reason* why no moisture appeared on the skin. A state of erection, distension, and perfect freedom, may be necessary to *perspiration*, and easily obtained in the living body ; but from the relaxation or compression

compression of the *villi*, any process similar to perspiration may be impossible, notwithstanding the action of *heat*, which could not make these vessels exert a power consistent only with *life*. The surface of the *cuticle* is always covered with an *unctuous*, or *oily secretion*; this is very conspicuous in the skin of the *Negro*, and makes it still more improbable, that watery fluids *soak* through it: this may be one reason why it does not suffer the *cutis* to dry,—though I doubt it. The cuticle of the hands and feet, I allow, in the living body, seem to imbibe moisture, and become softer; but it is probably, in consequence of its having *less living principle* than that of other parts.

That it allows of the *sweat's* passing through, may be easily accounted for, though the *soaking of fluids* through it should be denied; for admitting that in the palms of the hands, or soles of the feet, there may be *many layers of cuticle*, still it is most probable that the last
formed

formed corresponds in every respect to the first formed and intermediate *layers*, and that *pores* are opposite to *pores*, and connected with each other.

Besides, the *villi* appear to be *lengthened*, as the *cuticle* becomes thicker; I do not mean to say, that they were not originally intended to be long on the fingers and toes, and that those parts are not proportionably more vascular, even in the *fætus*, than almost any other part of the skin, but it is equally probable that the *villi* there were originally endowed with a property of *elongating* themselves in proportion to the necessity, since those parts, exposed to *greater friction*, would of course constantly be covered with a *thicker cuticle*. The *villi*, I know, are supposed to be longer there, for the same reason as on the *lips*; that is, for the purpose of more *exquisite sensation*. For though I have observed, that the greater part of the *villus* consisted of *blood vessels and absor-*

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bents,

bents, yet these have been supposed to be elongated on the account of the *nerves*. I should rather believe that several purposes, besides the accompanying the *nerves*, might be answered, by the *elongating* of the *villus*, and that a greater *perspiration*, for instance, as well as greater *absorption*, takes place on those surfaces. That they may be capable of *furnishing* a *thicker* and more constant *succession* of *cuticles*, is perhaps also part of the intention of *Nature* in forming them so *large* there. I have not seen any *villi* longer than those in the feet of *hooved animals*, as in the *horse* and *cow*, and the corresponding pores in the *hoof* are equally deep. Even in the *Slink calf*, these *villi* penetrate so deep into the *hoof*, that when the *foot* has been successfully *injected*, and the *hoof* afterwards separated by *maceration*, many of the *tore villi*, adhering in the *pores* of the *hoof*, have given it the appearance of being *injected*. On the *tongues* of *quadrupeds*, where the *cu-*
ticle

ticula and *rete mucosum* are much thicker than in any part of the human body, the *villi* are also larger and longer.

If the vessels elongate as the cuticle thickens, it will be said, what is the use of the cuticle's thickening at all in the *palms of the hands* of hard working people, or in the *soles of the feet* of those who walk much. It has been presumed, that it becomes thick, in these instances, in order to defend the tender vessels underneath from the effects of *pressure*, or *violent concussions*. I believe it does; (though I could conceive it *merely disease*) yet I would not deny that a porter's hand has as delicate a sensation of *touch* as a *lady's*. Though the vessels elongate as the cuticle thickens, still they will be better supported in passing through a thick elastic medium, and better able to resist the effects of pressure, especially as the *cuticle*, in thickening, becomes more *elastic*. That the vessels of the skin may be still more

defended against this pressure, is the reason we find such a quantity of *cellular substance* behind it; as for example, on the *heel, ball of the great toe, and buttocks*.

That *elasticity* in parts enables them to resist violence, need not *here* be explained. A man may catch a *cricket-ball*, if it flies with ever so much force, providing his hand *yields* on receiving it; if the *fixed* hand, on the contrary, were to receive the flying ball, it might shatter every bone in it.

Dr. *Hunter* has described and delineated, in the *London Medical Essays*, *white filaments* passing between the *cuticle* and *cutis*. These are most remarkable, in the *sole of the foot*, in the human subject. He suspects them to be *vessels of perspiration*, continued even to the *cuticle*. If they are vessels, it corresponds with my idea of vessels becoming larger and longer, in proportion as
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the *cuticle* becomes thicker. For these *filaments* are more easily demonstrated on the *heel*, or *ball of the great toe*, where the *cuticle* is *thickest*, than any where else. We have been informed, that it has lately been discovered, that these *filaments* were *nerves*. That the *nerves* never become larger, but on account of more *acute sensation*, or *greater action* in a part, appears to me a sufficient reason for rejecting the idea of *larger nerves* going to an *insensible* and nearly *passive* membrane. If these *filaments* are not *vessels*, from *analogy* to the other parts, of the internal surface of *cuticle*, I should rather suspect they were exceeding fine *processes* of the *cuticle* and *rete mucosum*, which line the *smallest pores* of the true skin; and if these *processes* are *elongated*, and go *inwards*, as the *cuticle* thickens, while at the same time they serve the same purposes as the *ducts of glands*, it comes to the same thing as if more of the *vessels* themselves had been *elongated outwards*.

If

If these *filaments* are really *processes* of the *cuticle* and *rete mucosum*, then I can demonstrate *three classes of processes* in these *membranes*. The *first* line the *pores*, through which the *hairs* pass; these are the *longest*, and generally have the *largest diameter*. The *second class* are easily distinguished on the inside of the *cuticle* which covered the *palms of the hands or soles of the feet*, (or indeed on any part of cuticle;) they line those *pores* described by Grew, and which Winslow calls, *the ducts of glands*; they are short, compared to the former, are transparent on the sides, and have a *white line* in the centre, which I do not well understand; they appear, in regular order, on those parts of the *cuticle* which correspond to the *parallel, or spiral ridges* of the *cutis*. The above mentioned *filaments*, perhaps, constitute the *third class*, are *longer* than the last, and more *slender* than any of the former.

In order to make it probable that *cuticle* is a substance, which may be pervaded by fluids, though it has *no pores*, *anatomists* have adopted one of two *theories*, respecting its formation. The *first* is, that it consists of the *callous extremities of the vessels of the skin*, reduced to this state by the *friction*, which perpetually takes place between the surface of the body and substances coming in contact with it. *Morgagni* adopted this opinion.

The *second* is, that *cuticle* and *rete mucosum* were originally and still are *exsudations of mucus from the ends of the vessels of the skin*; that this *mucus* was dried and hardened by the external atmosphere into a *membrane*. This last opinion has been supported by *Professor Meckel*, who observes, in confirmation of his opinion, that the *black membrane*, in the *rete mucosum* of the *negroe*, may still be dissolved in water, like *mucus* by *maceration*. I cannot persuade myself to be of either
 opinion.

opinion. There is something else in *cuticle*; nor does its known properties correspond with these theories. If the friction of external substances rendered the ends of the vessels of the skin *callous*, whence have we *cuticle* so perfect, in the earliest state of the tender *fœtus*, hanging in a *warm liquid*, more fit for *dissolving*, as one would imagine, than *producing callosity*? If the *cuticle*, on the other hand, is merely *concreted mucus*, whence should it remain months in water without *dissolving*, or becoming *putrid*? The *hooves*, *nails*, and *cuticle*, of animals, are supposed to be *similar substances*, and always come away together after *maceration* in water; yet the *hoof* in the *slink calf* is almost an *inch thick*, while the *cuticle* is nearly the same as it is afterwards in open air.

I formerly mentioned, that the *cuticle*, unlike dried *mucus*, neither in the living nor dead body, admitted of the *transuding* of fluids. Dr. Hunter observes, that

that the *fine membrane* in the *rind* of fruit, such as *lemons* and *oranges*, has the same property, as is demonstrable from the drying and shrinking of the fruit, when this membrane is removed, whereas, if it is kept entire, the fruit may be preserved for many months.

I cannot well suppose any part of the skin of a living animal *inorganic* and *not possessed of life*. If the *cuticle*, tho' an insensible membrane, were not *alive*, and possessed of *irritability*, why should touching it with *caustic*, which deprives other parts of *life*, and makes them drop off, have the same effect on the *cuticle*?

If a bit of *cuticle* is touched slightly with moist *lunar caustic*, it soon becomes *black*, and in a day or two drops off, shewing a new surface in every respect like the former. I do not admit that this is *new cuticle*, so quickly regenerated, but the *cuticular surface of rete mucosum*,
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which

which has the same appearance, and the same properties, as the *cuticle*.

Spirit of nitre dropt on the *cuticle*, turns it *yellow*, and produces, though more slowly, the same effect as the *lunar caustic* did.

The *substance of the teeth*, like the *cuticle*, has been supposed to have *no vessels*, though it was *originally* deposited by *vessels*; and there are several circumstances which favour this opinion; yet in attempting to *saw a tooth* in the living body, the patient complained of *pain* the moment the *saw* got through the *enamel*. If there are *nerves* in the bony part of a *tooth*, there can be no doubt of its *also having vessels*.

Cartilages covering the ends of bones, in the full grown animal, have not the least *vestige of vessel* that can be demonstrated; but cartilage may be *absorbed* as well as *bone*; and if in the diseased

eased state, it is most probably *vascular*, it must have been *so* in the sound state.

Having formerly observed, that the *brain* steeped for months in *spirit of sea salt*, instead of being *dissolved* like the *muscular flesh*, or like the *viscera* of the *thorax* and *abdomen*, become *harder* and *firmer*, * I wanted to see what effect *concentrated acids* would have upon the *cuticle*.

I took a piece of the *cuticle* of a *child at birth*, and divided it into three por-

* It not only becomes firmer, but its *fibrous texture* may thus easily be demonstrated. The *nitrous acid*, however, dissolves it *entirely*, and the *vitriolic* converts it *partly* into *pulp*.

Not only the *brain*, but the *nerves* also, appear to have *other properties* than we have hitherto apprehended. *Some years ago*, I demonstrated, by *experiments on living animals*, that *nerves divided* unite again; and that when *portions had been cut out*, they were *regenerated*.—In both instances the animals perfectly recovered.

These experiments I hope soon to be able to lay before the *public*; mean time I am happy to find, they have been recently confirmed by so great authority, as that of the *Abbè Fontanà*, to whom I communicated my discovery, and shewed my preparations of united and regenerated nerves.

tions ; each of these might be about *an inch square*, and were put into separate glassess, and spread out ; two drachms of *vitriolic acid* was poured upon the *first* ; two drachms of *nitrous acid* upon the *second*, and the same quantity of *spirit of sea salt*, upon the *third*. After they had remained *an hour* in the acids, I found that they were *not dissolved* ; I washed them in water, and examined them particularly ; — *That* which had been put into the *vitriolic acid*, had, in some degree, *lost its colour*, and was become *brownish*, but was not however in the least *dissolved*, and the *processes* were still exceedingly distinct ; it was not *more tender* to the *touch*, or to the *endeavour* to pull it *asunder*, than before ; nor had it lost its *elasticity*, except in a small degree. — *That* which had been put into the *nitrous acid*, though it was not dissolved, had *split* into *different pieces*, and was more *pulpy* than before, like a piece of *cuticle* from the sole of the foot, *macerated in warm water* ; it was
also

also more tender, did not bear *handling* so well, but the *processes* and *natural texture* of the skin were still apparent; it had lost nearly all *elasticity*.—That on which the *strong spirit of sea salt* had been poured, appeared to have *suffered least*, and had not even lost its *colour* in the smallest degree; it had not lost its *elasticity* in any degree; nor was it apparently altered as to its *texture*; the *minute processes* themselves had undergone no change, and it could be *handled* with as little injury as before; these are properties in the cuticle which by no means correspond with *callous vessels* or *concreted mucus*. A very remarkable circumstance in one of these experiments was, that though the *nitrous acid* gives the *cuticle* a *yellow colour*, if it touches it while it adheres to the *living body*, it had no immediate effect of this kind upon the *separated cuticle*; nor did I perceive that it was *yellow* till next morning, after it had been *many hours* in water. I repeated
these

these experiments with cuticle steeped for an hour in oil of tartar, per deliquium, least any thing oily on the cuticle might have prevented the acid from getting in contact with its surfaces ;—the event was the same.

My suspicions that the *cuticle* was *organized* have been still further confirmed, by some new observations I have made on *skin*, injected, with a view to shew the appearance of the *small-pox pustule*. — I have now more reason to believe, that the *cuticle*, like some parts of the *conjunctiva of the eye*, though it cannot be *injected* in the sound state, was originally *vascular*, and circulated the *red blood*. It may still have *vessels* carrying *transparent fluids*, and I would not altogether deny, that *those vessels* might not sometimes be again dilated, so as to be capable of receiving the *red blood*, or our *injection*. — The *hairs* themselves, though *reputed* to be *inorganic*, like the *cuticle* and *nails*, or like the

the hoofs of animals, are notwithstanding said sometimes to bleed so as to endanger life, in the disease termed *plica pollonica*.

Though I have not seen vessels in cuticle or rete mucosum, I have successfully injected a membrane between rete mucosum and the cutis, in the skin of those who have died of the small-pox.— This membrane I discovered in consequence of a conversation I had with Mr. Baynham, of Virginia, at the time he shewed me some preparations of cutis, in which, he believed, he had injected rete mucosum. I was surprized at the appearance of vessels in his membrane running parallel to the surface of the skin, and which formed a net-work. I was not perfectly satisfied however even then, that it was rete mucosum he had injected. Mr. Baynham was so obliging, as to let me have his preparations home, and desired I would examine them carefully, and prosecute the subject if I pleased.

After

After some time spent upon this subject, I was still at a loss what to conclude of his *membrane*. I saw that it was *certainly, not rete mucosum* which I observed had already been *previously turned down*, and was still adhering to the inner surface of the *cuticle*. This *membrane* was much *thicker* than I conceived *rete mucosum* could be ; it was *exceedingly tender and pulpy*. The *surface* from which it was removed, as well as its own *internal surface*, were *rough* ; nor did the *surface* of the *skin* appear more *porous* than it was before.—As I could not tell what to make of it, it induced me to make some *similar preparations*, and gave me an opportunity of discovering a *very beautiful vascular membrane in the injected small-pox skin*, situated in the same part with Mr. Baynham's, that is, between the *rete mucosum* and *cutis*. Mr. Baynham's preparations, he informed me, were made from the skin of a leg *where there had been an exostosis of the thigh*, and of consequence, a *derivation of more blood*

blood to the *skin* than usual.—He had plunged the *skin*, after it was injected, into *boiling water*, for a few seconds, and afterwards macerated it in *cold water* for several days.

I had no opportunity of making *experiments* upon *similar skin*, but I had many pieces of injected *small-pox skin* in *spirits*. Mr. Baynham had informed me, that he used the *boiling water* to *thicken the membranes*, and make them *bear the being handled* better. I thought the *spirits* would have an equally good effect. I macerated those portions of *skin* in *putrid water* for a week, during the heat of the *summer*; the *spirits* with which they had been *previously impregnated*, made them resist the effects of *this water* longer. *Cuticle* and *rete mucosum* were already turned down; and upon the *eighth* or *ninth day* I found I could now separate a *vascular membrane* from the *cutis*, in which were also situated the *injected small-pox*

A a *pustules.*

pustules. These last consisted of *circles* of long floating villi at the circumference, but of a white uninjected substance in the center. This central part Mr. Hunter had previously said, was a *slough*, formed by the irritation of the *variolous matter*. The surface of the skin from whence *this membrane* was separated, was *elegantly porous*. The pores now appeared *exceedingly more numerous*, and this surface of the skin was still *tough and shining*. From the vast number of pores now visible I inferred, that the *processes* of the *cuticle* and *rete mucosum* must be also more numerous than we are aware of; and many of these *processes* must be *invisible* in the microscope, from their *exility* and *delicate texture*, though their corresponding pores are visible. But as the *processes* of the larger pores are visible to the naked eye; and as Dr. Hunter's white filaments are not discoverable to the microscope, after they are once tore through, the *invisible processes* I contend for,

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most probably exist, and may be the last-mentioned *filaments* themselves.—I macerated the *same skin* for four or five days more, and separated *another membrane* more delicate than the *former*, but also vascular; the *former* I easily preserved; the *latter*, attracted by the instrument which separated it, or unable to bear the *agitation* of the *water* or *spirits* in which it was separated, constantly broke down; but the corresponding surface of the skin was still *tough* and *shining*; the *pores* were now much *larger* and *more distinct* than before, and convinced me that the *appearance* was *natural*, and that the skin had sustained no real injury in the *process*.

Was I to describe the *different membranes* which lie on the surface of the *true skin*, I should now say they were *five*, each of which I conceive is a *cuticle*, or to become a *cuticle*.

The three first, are evidently cuticles, and the two last, most probably, are forming into cuticle, and, like the second and third, are to succeed the first, which is perpetually falling off in small portions, like scales,—the only circumstance which seems to favour Lewenhoeck's doctrine, that the cuticle is formed of scales.

If I am still not perfectly understood respecting these *five membranes*, I repeat, that *cuticle*, commonly so called, makes the *first*; the *rete mucosum* is double and makes the *second* and *third*; the *first vascular membrane* in which the *small-pox pustules* are chiefly seated, makes the *fourth*; and the *membrane*, which may be separated some days after the separation of the last, by continuing the *maceration*, and which shews the *pores still larger*, makes the *fifth*. These *two last membranes*, I fancy, might easily be detected in the skins of those who died of the *measles*, *scarlet fever*, or other *eruptive diseases*, as well as in the *small-pox skin*; for I conceive,

ceive, that these *eruptive diseases* do not create, but demonstrate these membranes, in consequence of the great determination of blood, in these cases, to the *skin*.

I mean to prosecute the subject, and if any observations I may be able to make upon the *skin*, will throw any light on the seat of *eruptive diseases*, or help the physician more readily to distinguish them upon their *first appearance*, I shall be sufficiently rewarded.

I have within these few weeks, procured portions of *skin*, under the same circumstances, exactly, as Mr. Baynham's.—I have been able to separate a vascular transparent membrane, smooth on both sides, and more like that which I removed from the *small-pox skin* after *cuticle* and *rete mucosum* had been turned down, than Mr. Baynham's; but, as it has left the surface of the *cutis* rough, I am not perfectly contented even with
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my own preparations; they have not erased every doubt in my mind respecting Mr. Baynham's membrane; I am convinced that it is something more than the surface of the *cutis* itself become tender, from the previous plunging it into boiling water, and subsequent macerations in cold water, separating into two layers.— For I own, it separated of itself, without any force, but the roughness of the separated surfaces, with the tender pulpy state of the membrane, carry some suspicions of a partly dissolved instead of merely separated lamina. Though I think that Mr. Baynham has separated a part of the tender surface of the *cutis*, with his membrane; yet as the reticulated appearance of the vessels, in the external surface of that membrane is the same with that which I have seen in the small-pox membranes, and in skin, similar to that which he had prepared, I must believe, that one of the vascular membranes I have seen, and Mr. Baynham's are at bottom, the same; and must, therefore, still consider him as the first dis-

coverer

coverer of the *cuticula quarta*, though I do not admit that *he* has injected *rete mucosum*. Mr. Baynham will do me the justice to believe, that could I have talked more favourably of his *preparations*, I would most willingly have done it, the open, unsuspicious manner with which he treated me, on my visiting him, and his frankly trusting the affair with me, must have pre-engaged my disposition to oblige him on this occasion. From the specimens I have seen of his *anatomical abilities*, and from that known ardour with which he pursues his *medical enquiries*, I have no doubt of his becoming more deservedly eminent, than if he had actually injected *rete mucosum*, and of course done what Buysch himself could not do.

Remarks on insensible Perspiration.

Albinus and *Mekel* had both supposed, that the perspirable matter passed through
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an imperforated cuticle, in the form of vapour; the former supposed that when it appeared sensible, in the form of sweat, it was in consequence of its being condensed on the surface of the body. They compared the oozing of this vapour through the cuticle, to the steam of warm water passing through leather.

If perspiration takes place, said I, through an imperforated cuticle, so must absorption from the skin.

Their ideas were ingenious, but I could not reconcile my mind to either proposition.—For *sweat* is frequently most copious when there is least time allowed for the condensation of the insensible perspiration; nor has it been proved that the surface of the body is then colder, or more capable of condensing this vapour, than at other times.

Though I knew that the rays of light, could pass through glass, in which
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there are no *pores*, yet I could not discover any resemblance in *vapour* to *rays of light*, or that *glass* was, in any respect, like *cuticle*.

Boerhaave observed, (as every body else must have done), that though the *vapour* of the *lungs* in *expiration* was in the *summer's heat* invisible, it became perfectly *distinct* when it was condensed by the *winter's frost*.

He observes, that if the hand is introduced in summer into the *powdered ice* of an *ice-house*, it *smoaks* and gives the same appearance as the *breath* does in *winter*: he amuses himself with the *idea* of *winter's cold* being *instantly* produced in the midst of a *summer's assembly*; each *individual* would then appear, says he, like a *beaten deity*, wrapped up in his own cloud.

He says, that by thrusting the naked arm into a long narrow *glass vessel*,

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the *insensible perspiration* also becomes *sensible*, in the same way that the *vapour of the lungs* becomes *sensible* by *winter's cold*, or *breathing on a mirrour*. The *cold*, in these instances, *condenses the vapour*, as *cold water* thrown round the *worm of a still*, does the *steam in distillation*. He adds, that it was astonishing how much *limpid fluid* could, in this way, be collected, but says nothing of the *particular quantity*, nor seems to have attended to any thing further than the *insensible perspiration's* becoming *sensible*. Winslow says, that he could demonstrate the *insensible perspiration*, by opposing his naked head to a *white wall* in a fine summer's day; this *vapour*, he says, will then become *visible*, (magnified by the sun's rays), and appear *ascending like smoke*.

I wished to know, whether *this vapour* would become *equally sensible* through *leather*; and pervade it, in the manner it was *supposed* to pervade the *cuticle*.

Sanctorius,

Sanctorius, in a series of experiments, weighing himself daily for *thirty years*, with a view to determine the *quantity* of the *insensible perspiration*, did not take into his *calculation* the *insensible absorption* from the *atmosphere*; and might frequently be attributing that to *checked perspiration* which belonged to *insensible absorption*. It was also thought, that he made the *quantity* of the *insensible perspiration*, in *twenty-four hours*, greater than it *possibly could be*. I thought I might be able to come *nearer the truth*, by weighing the *actual vapour* of *insensible perspiration*, after it was *condensed* into a *fluid*.

I wished besides to know what *affinity* there was between the matter of *insensible perspiration* and the *vapour* of the *lungs*.

The *vapour* of the *lungs* (or the *breath*) was said to be *fixable air* and
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water : others asserted, that it *also* contained *phlogiston*.

Having proved *formerly* that the *calces* and *salts* of *mercury* were revived into *quicksilver* in the body ; and having constantly observed, that almost every thing we eat or drink contains *phlogiston*; that the *vapour* of the *intestines* was *inflammable*; and that there was much *electric fire* in the body : I thought this *latter opinion* was *more than probable*.

Though many of the properties of *phlogiston* are *known*, yet as it cannot be procured *uncombined* with something else, and in the *simplest forms* it can be procured, seems frequently unwilling to leave the body, to which it is united, to combine with another that may be presented, unless that body is placed with it in some uncommon situation, such as in a *red heat*, or exposed

posed to a *powerful acid*, it is for *these reasons* still not *perfectly* known.

Before many readers will understand me, it will be necessary to premise a few observations on *fixed air* and *phlogiston*.

Calcareous earth, burnt in the fire, looses a *something*, which, when united with *air*, renders it *fixt*, and in consequence of this, becomes a *calx*, or *quicklime*. Before it was *burnt* it was *insoluble* in water; now that it is *lime*, water *dissolves* a certain proportion of it. If *fixt air* is added to *lime-water*, the *lime* which had combined with the water, and was *invisible*, now attracts the *fixt air*, becomes *calcareous earth* again, and as *insoluble* in water, is *precipitated* in *fine flakes*, which, for a while, are suspended in the water, and give it a *milky appearance*, but on standing, soon fall to the bottom, as a *powder*. If more *fixt air* is added to the water, the water acquires a power of dissolving *calcareous earth*

earth; takes up the powder which it had let fall, and again becomes *transparent*; or if, instead of adding more *fixt air* to the water, some *spirit of sea salt* is added, the *calcareous earth* effervesces with the *acid*; a combination is formed, and the liquor becomes also *transparent*. *Fixt air* is formed, by the *fermenting* of *saccharine fluids* into *vinous*; by the combination of *acids* and *calcareous earths*; the combination of *acids* and *alkalis*; by the *burning* of fuel, and by the *respiration* of animals.

It is heavier than *atmospheric air*, and deposited in a vessel at rest, remains there for some time.

It will not serve for the purposes of *burning* or *respiration*; a *lighted candle* introduced into it is *instantly extinguished*; and *an animal*, if he can have no other *air* to breath, *dies immediately*.

Fixable air concentrated, turns the *infusion of purple or blue flowers* red.

It

It also gives *acidity* to water, and has, for amusement, been employed instead of *lemon juice* to make *punch*.

Metalic bodies, when exposed to a certain degree of *heat*, lose *phlogiston*, and are also said to become *calces*.

Phlogiston is that *principle*, which, in passing from some bodies, and combining with *air*, occasions, frequently, *flame*. Thus, *a candle burning* gives over its *phlogiston* to the *air*, and as *effervescence* is the mark of an *acid* and an *alkali's* uniting, so *inflammation*, or *burning*, is often a mark of *phlogiston's* combining with *air*.

Many bodies contain *phlogiston*, and are, notwithstanding incapable of *inflammation*. *Volatile alkali*; most of the *metals* themselves, are of this class.

The metals *calcine* in common air, or part with their *phlogiston*. Thus
iron

iron parts with it most readily, in what is commonly called *rusting*, but does it so slowly, that no *heat* or *inflammation* is perceived.

Metals also part with their *phlogiston* in combining with *acids*; thus *copper filings*, in combining with *nitrous acid*, parts with its *phlogiston* and forms *nitrous air*.

Sulphur set on fire, parts with its *phlogiston* to the air, and leaves the other part of its composition, the *vitriolic acid*, behind;—so does the *phosphorus of urine*, leaving also *its proper acid* behind.

Charcoal contains it, in great quantity, and all *inflammable bodies*.

Phlogiston, united with air, unfits it for *inflammation* of inflammable bodies, or the *respiration* of *animals*; a *lighted candle* is extinguished on immersing it in *this air*; and an *animal* exposed to it, *dies suffocated*.

Phlogiston,

Phlogiston, added to substances, is said to make them *lighter*, and is the only *substance*, which is *believed* to repel the *centre* of the *earth*.

Some of the most eminent *chymists* have doubted, however, of this *last property*.

With these ideas of *insensible perspiration*, *fixable air*, and *phlogiston*, I made the following experiments.

EXPERIMENT I.

ABOUT ten in the morning, the thermometer at 67° in the shade, and 71° in *my apartment*, my pulse beating 65 in a minute, having taken little or no *exercise*, and *feeling* perfectly well, I washed and dried my hands, and introduced my *right hand* into a *clean empty bottle*, capable of containing *three pints and a half*. The mouth of the

C c

bottle

bottle readily admitted my hand ; I had previously taken a dried bladder, and cutting off the bottom and upper part, had made it into a *hollow cylinder* ; this had been *wetted* and drawn on the neck of the *bottle*, like a stocking, for some way, and was allowed to dry to the glass ; the middle and opposite end were also allowed to dry as a *hollow cylinder*, except at that part where it was to be fixed to my wrist, by a *ligature*. Having made this *ligature*, I observed, in *less than a minute*, that the inside of the *bottle* was become *dim*, as it would have been had one held it over the *steam of warm water*. In about *ten minutes*, *small drops* began to appear on the *bottom and upper side* of the *bottle* which was held in the *horizontal position*, and equally covered with a *thin wet piece of linen* ; this was moistened from time to time during the experiment, that by the *cold* the *evaporation* from it produced, the *vapour* in the *bottle* might more readily be *condensed*. After keeping my hand in this situation

situation *an hour*, I found I had collected a *tea spoonful* of *transparent* and *perfectly insipid fluid*. This *fluid* I poured into the *scale* of a balance, which had in its *opposite scale*, a *weight*, equal to the *weight of a bit of dry sponge*. With this *sponge* I absorbed the *remaining fluid* in the *bottle*, and put it into the *scale* with the *former fluid*. The *fluid* I had thus collected weighed *thirty grains*. This experiment I repeated several times, and in general with the *same effect*.

The greatest part of the fluid was collected by the upper side of the bottle, and the vapour *seemed* to have a greater tendency to *ascend*. This, however, might be owing to some circumstance which made the upper side of the bottle *colder* than the *under*, though I was not aware of any that would have this effect. A quantity of *lime-water*, equal to the *fluid* collected in the bottle, weighed *thirty-nine grains*.

A *lighted wax taper*, introduced into the *bottle*, at the *end* of the *hour*, before the *fluid* was removed, and immediately on withdrawing my hand, (*which was done very gradually*), was *not extinguished*, though it burnt *dimly*.

If my hand is to the rest of the surface of my body, as one to sixty; and if every part of that surface perspired equally with my hand, then I lost, during that hour, by insensible perspiration from the skin, *three ounces and six drachms*; and in *twenty-four hours*, at that rate, would have lost *seven pounds six ounces*.

EXPERIMENT II.

I repeated the foregoing experiment some hours after, walking gently in open air; at the end of the hour, the collected fluid weighed *forty-eight grains*. This experiment also was repeated with the same effect. From this I inferred,
that

that the *insensible perspiration* was increased *two-thirds* nearly, during *exercise*; the *whole surface of my skin* lost in *this hour* *six ounces*; and at that rate, in *twenty-four hours*, would have lost *twelve pounds*. Hard working people, very probably, lose *still more*.

EXPERIMENT III.

I repeated experiment first at nine in the *evening*, thermometer 62° , the *collected fluid* weighed only *twelve grains*.

The *insensible perspiration*, then, is different under different circumstances. This quantity, however, was the smallest I ever obtained in these experiments.

The *size of the body*, the *quantity of food* taken in, the *vigour* with which the *system* is acting, the *passions of the mind*, *external heat or cold*, are circumstances which will ever occasion *considerable variety*

variety in the quantity of the insensible perspiration.

EXPERIMENT IV.

I breathed for an hour into the *same bottle* which I had formerly used for the experiments with *my hand*, and under the same circumstances. *I inspired fresh air, and breathed into the bottle; as I supposed that some of the air of expiration would be returned from the bottle, and that of course, all the vapour would not be condensed, I breathed more forcibly into the bottle than I would have done in ordinary respiration. The process, especially towards the end, was exceedingly painful, and almost tempted me to give up the experiment. I believe it was the spoiled air in the bottle that affected me every time I brought my mouth to it to expire. Notwithstanding of this, I repeated this experiment next day, and with the same effect;*
that

that is, *at the end of an hour*, I had collected *a hundred and twenty-four grains of insipid transparent fluid*. The fluid obtained in one of these experiments I poured into *lime water*, but it produced *no change* on it. Notwithstanding the uneasiness I felt in breathing into the *bottle*, a *lighted wax taper* introduced into it at the end of the hour, *was not extinguished*.

If I lost *a hundred and twenty-four grains of vapour*, by *respiration*, in an hour, at the same rate, I should have lost *six ounces, one drachm, and thirty-six grains*, in *twenty-four hours*; which, added to the former *cutaneous exhalation*, would make the *whole insensible perspiration* in *twenty-four hours*, equal to *eight pounds, one drachm, and thirty-six grains*; and the *evaporation* from the *lungs*, will be little more than *one fifteenth of the whole*.

Sanctorius supposing, that a man took into his stomach *eight pounds of liquid*

liquid and solid in twenty-four hours, allowed three pounds of this to pass off by stool and urine, and the other five he laid to the account of the insensible perspiration: the evaporation from the lungs he calculated at one-sixth of the whole.

It is more than probable, that when the body weighed heavier in Sanctorius's experiments, than he expected to have found it; a circumstance which he attributed to checked perspiration, that no small part of this weight was to be laid to the account of increased insensible absorption from the atmosphere.

I know of no experiments which tend to ascertain the precise quantity which is absorbed from the atmosphere; nor do we know whether this absorption is constant or periodical. There are some observations which prove, that plants absorb from the atmosphere; indeed I cannot conceive, that a body endowed with the property of absorbing, should be constantly surrounded with moist and fluid air, and not absorb it.

Sanctorius

Sanctorius was thought to have allowed *too much* to the effect of *insensible perspiration*, and the *air* of *Italy* being warmer than *ours*, alone made his *calculation* appear *probable*. He appears also to have allowed *too little* out of eight pounds of food for the loss by *urine* and the *intestinal discharge*. I have made the *insensible perspiration* still more, and those who do not take one half of *his supposed quantity* into the stomach in twenty-four hours, will think the *proposition absurd*. These readers will please to reflect, however, that more goes into the body than they know of; and admitting *insensible absorption* from the *atmosphere*, appears to me *fully sufficient* to solve this problem.

We have instances of people's making a ten times greater quantity of *urine* than the *liquids* they drank; and I formerly observed, that *De Haen* was convinced, that the water of *ascites* was frequently accumulated by *absorption* from the *atmosphere*. An eminent philosopher, who

weighs himself several times a day, in a very accurate *ballance*, informs me, that *soon after an evacuation from a purgative*, he has weighed *some ounces heavier than just before it*.

EXPERIMENT V.

I breathed through *lime water* in a *curved glass tube*, the water immediately became *turbid*; and though on continuing to breath through it, it *once* became *less turbid*, yet it never became *transparent*, though the breathing through it was continued for *an hour*. On adding some *spirit of sea salt* to it, it presently became clear. When *fixt air*, I have said, is added to *lime water*, it becomes *turbid*, but on adding more *fixt air*, the *calcareous earth* is dissolved, and the liquor becomes *perfectly transparent*. There is therefore something else in the *air of expiration*, than that *something*, which, added to *air*, makes it *fixt*.

EXPERIMENT VI.

I introduced into *lime water* some air, in which a *wax taper* had extinguished itself; the *water* instantly became *turbid*; no further addition of this air rendered it *transparent*; though it became *less turbid*, as in the former experiments; and *spirit of sea salt*, now added, made it *transparent*. This air, I own, contains *fixt air*, but it surely contains *more phlogiston*. *Phlogisticated air* and *fixt air*, it must have been observed, wonderfully correspond in several of *their distinguishing marks*. They are both *unfit* for *respiration* and *inflammation* (or *burning*.) Though they do not *tally* in every circumstance, may not this depend on the *difference of situation*, or some difference in the *mode of combination*? A *diluted*, or *weaker acid*, will not produce the effect of a *concentrated one*. The natural colour of *spirit of nitre* is *yellow*; add a little *water* to it, the *yellow colour* still

remains ; add a *little more*, it becomes *green* ; and add much water it becomes *transparent* ; all the while it is still *spirit of nitre and water*.

EXPERIMENT VII.

I introduced into *lime water* some *air*, in which *burning phosphorus of urine* had *decomposed itself*, and shook them together ; the *lime water* was instantly *decomposed* ; no additions of this *air* made it *transparent* again ; but on adding some *spirit of sea salt* it became *transparent*. The *phosphorus of urine* is allowed by the chemists, to be the *nearest to pure phlogiston* of any substance. This experiment seems to prove, that *phlogiston* will produce the same effect on *lime water* as *fixable air*, and confirms the *suspicion* I have entertained, that *phlogisticated* and *fixed air* are, at bottom, the same.

Dr. Priestly found, that the *electric stroke* received over the surface of *lime water*

water, occasioned a *precipitation* of the *lime*.

EXPERIMENT VIII.

I repeated experiment first, and threw the *fluid* so collected into *lime water*, it produced no change in it. I threw some *lime water* into the *bottle* where my hand had remained an hour, after some *agitation* the *lime water* became faintly *turbid*.

EXPERIMENT IX.

I made a similar experiment to the first, with my *foot* instead of my hand; of course, employed for this purpose, a *larger bottle*. The *fluid* collected produced no change on the *lime water*; but *lime water* thrown into the *bottle* and agitated, became as *turbid* as when the *air*, in which the *wax taper* had extinguished itself, was mixed with it.

Least the *stagnating* of the *perspirable matter* in the *stocking*, and its *fermenting*
might

might be suspected to have *generated* the *fixable air* apparent in this experiment, I must observe that my *foot* was *previously washed* in *warm warm*.

This last experiment I repeated several times, and with the same success: from these I inferred, that (admitting the *common theory* of *fixed air* and *phlogiston*) *something* passed off with the *vapour* of *insensible perspiration* by the *skin*, which rendered *air fixt*. As this *something*, added to *air*, makes it *heavier* than *atmospheric air*, it should have been taken into the *account* of the *weight* of the body in *Sanctorius's* experiments. If *phlogiston* passes off at the same time with the *perspirable matter*, along with *that* which, in making *air fixt*, makes it heavier; and if *phlogiston* really *repels the centre of the earth*, and is the *principle of levity itself*, then, the one may counterbalance the other, and *Sanctorius's* experiments, as far as *phlogiston* and *fixed air* are concerned, may be still pretty near the truth. If the re-
spired

spired vapour from the *lungs*, during *exercise*, is in the same proportion with that from the *skin*, under that circumstance, then the *whole* of the *insensible perspiration* will be *still greater* than I have made it.

EXPERIMENT X.

I introduced my hand, covered with a new *shammy leather glove*, into the bottle, as in *experiment first*, and under similar circumstances. In an hour I collected *twenty-four grains* of *insipid transparent fluid*; if the *glove* may be supposed to have absorbed *six grains*, which is very probable, then I collected nearly the same quantity as in *experiment first*.

EXPERIMENT XI.

I introduced my *foot*, with a *rigidly dry boot* on it, into a *large bottle*, and
went

went through a process similar to *experiment first*. It was *long* before *any vapour* appeared on the sides of the *bottle*, but before the end of the *hour* there was *some appearance* of *dimness*, and *very small drops*.

The *vapour* of *insensible perspiration* then, passes through *leather*; and it is very fortunate it does, since we may *thus* defend ourselves against the injury of the weather, and *perspiration* continue *undisturbed*. But though it pervades *leather*, which is a *dead porous substance*, yet I cannot believe that it *pervades* in the same manner the *living cuticle*. The fluids do not *transude*, or *soak*, through *living membranes*; nor can I believe that even *vapour itself*, penetrates an *imperforated living cuticle*. The *vapour* passed *with difficulty* through the *boot*. It is said, that *dragoons*, who *constantly wear boots*, have *small legs*. If *atmospheric absorption* is equally retarded on these surfaces, their *growth* may be *prevented*

vented like that of *trees too closely planted together*, and from the same cause. The *pressure* of the *boot* preventing a *full exercise* of the *muscles*, is also to be included.

The *evaporation* from the body, does not appear to me to be like that from *dead matter*. It took place *within the bottle*, nor was in the least *interrupted* though the *air* continued the *same* throughout. Were I to compare it to any thing, it would be to the *steam* which a *torrent* in falling over the *brow* of a *rock*, in its *rapidity*, *flings off* in a *floating cloud*, while the *torrent* itself holds on its way.

The *impetus* of the blood, and the *relaxation* of the vessels of the *skin*, are as certainly necessary to the passing of the *insensible perspiration*, as to the passing of the *sweat* itself.

It may, perhaps, sometimes take place like the *sweat* in fainting or dying animals, from relaxation of the vessels only.

If *phlogiston* passes off from the surface of the body in *perspiration*, then, living animals, and bodies on fire, are, in some respects, in the same situation, and are both giving off *phlogiston* to the atmosphere.

That the blood contains *phlogiston* there can be no doubt. That it burns with a blue flame when dried; that in this state it revives the calces of metals, if exposed with them to a sufficient heat; that papers dipt in the serum of the blood when it happens to be of a white colour, dry greasy; that an oil is obtained by the distillation of the blood; as well as Dr. Priestly's ingenious experiments prove this.

That the blood parts with *phlogiston* in *respiration* I would also admit. I have a strong suspicion that it is this which converts the inspired atmospheric
air

air partly into fixable air. Be that as it may, I am convinced there is something more in respiration than the merely separating phlogiston from the blood.

Respiration is not only necessary to the free circulation of the blood through the lungs, but the stimulus of the atmospheric air on the lungs has a very considerable effect, in continuing, and frequently in re-producing the heart's motion.

In presence of several of my anatomical friends, I opened the windpipe of a dog, whose spinal marrow had been divided in the neck, in whom the par vagum and intercostal nerves had also been divided at the same place. He had been apparently dead above a minute, and the heart had ceased pulsating. I introduced a large blow-pipe into the opening made in the windpipe, and began inflating the lungs. This I did, in such a way, as to imitate full and slow respiration. In about half a minute, the heart began to pulsate again;

I continued my *inflation* of the *lungs*, and the *heart* continued to beat full equable pulsations, at the rate of *seventy* in a minute, for a compleat half hour.

The air thrown into the *lungs* was here *phlogisticated* air. It contained undoubtedly *fixable* air, for, by one *in-spiration* into a large bottle containing half an ounce of *lime water*, on agitating it, I can at any time render the whole turbid and white as milk. The *heart's* action was re-produced and kept up from the *stimulus* of the air on the *lungs*, and in consequence of keeping up their motion.

I will not affirm that it is the *stimulus* of bad air, which obliges the muscles of the *larynx* to contract so as to shut up the *glottis*, in suffocation from the vapour of burning charcoal, &c. but I believe it is nothing else. I have once or twice been nearly choaked from the skin of a currant berry, happen-
ing

ing to get between the *root of the tongue* and the *basis of the epiglottis*. I breathed as in a *fit of the asthma*, and saw my *face turgid with blood*; this continued till a *gulp of water* washed the *skin* away.

My ingenious friend Mr. Crawford, has proved that *atmospheric air* contains an *astonishingly greater quantity of absolute heat*, than a *mixture of fixed and phlogisticated air*, or the *air expired from the lungs of animals*. It is probable, that the *heat* which *atmospheric air* loses in the *lungs* on becoming *fixed and phlogisticated*, may give *some stimulus* to the *system*, but there is still *something more* I am persuaded in *respiration*.

From *this long digression on the skin*, and *insensible perspiration*, I once more return to *absorption in general*.

IV. I shall next suppose, that I am desired to give *some account of the manner*
in

in which absorption begins, and is afterwards carried on. Here, I must own, that many things are conjectures. Leiberkuhn, whose description of the beginnings of the lacteals, is something like mine, supposes a great part of the villus to be of the nature of sponge; he describes the orifices which I have seen, but makes them less numerous, observing, that it is seldom there is more than one orifice to the spongy body, which he calls ampullula, and which he considers rather as an appendage to, than the beginning of the lacteal. Now, in the bulbous extremity, which I have delineated, there appeared about twenty or thirty orifices. Again, he leaves you to suppose that the ampullula as a sponge drinks up the chyle, and that from the ampullula the chyle is somehow squeezed into the orifice of the lacteal, which lies behind it.

Haller, in his Physiology, and Dr. Fordyce, in his Natural history of the human body, adopt the theory originally suggested

suggested by *Aggiunti*, which compares the *absorbent vessel* to a *capillary tube*, and supposes that *absorption begins* in consequence of a *power* in the *vessel similar to capillary attraction*; that after this, it is carried on by the *muscular powers* of the *absorbent*. There are some objections to this theory.

In order that *capillary attraction* may take place, nothing more is wanted than a *certain diameter of tube*, and that the *extremity of the tube be immersed in a fluid*.

Before *animal absorption* can take place, there must be *something more*; there must be a *particular stimulus* on the orifice of the vessel, otherwise it will *not absorb*.

Besides, a *capillary tube* having once taken up *water*, will not take up *oil*, or having taken up *oil*, will not admit *water* to rise in it, whilst the *human absorbents* equally take up *oily or watery fluids*.

Mr.

Mr. Hunter is disposed to consider the *absorbent vessel* in the *light* of a *living animal*, and thinks it may take up *fluids* or *solids* as a *leech*, for example, or a *caterpillar*, take up their food.

A Gentleman who some time ago published on the *Circulation of the blood*, supposes that *absorption* begins in consequence of *inanition* about the *heart* and *great vessels*. I am not sure if I understand him *right*; if he means that there is a *vacuum* formed *there*, then *his idea* of *absorption* in the *intestinal tube*, is something similar to that entertained by the *philosophers* who adopted the theory of the *fuga vacui*, whilst *absorption* on the *surface of the body* with him must depend upon the *same principle* with that by which *water* raises in a *pump*, where not only the *vacuum* formed, but the *pressure* of the *external atmosphere* is taken into the account. I do not doubt that the *body* *absorbs* more after *evacuations*, and in proportion to the *necessity*; but I doubt

general *stimulated contract*, and *having contracted*, if in a sound state, must from their own nature be *presently relaxed*: whatever is to be absorbed, is applied to the absorbing surfaces, either by the *pressure* of the *external atmosphere*; by the *peristaltic motion* of the *stomach* and *intestines*; by the *motions of respiration*, *pulsation of neighbouring arteries*, or in short, by the *contraction of muscular parts* in general.

This *matter* coming into *contact* with the *orifice of an absorbent*, *stimulates it*; the first effect of this *stimulus* is to make it *contract*; it of course takes up *less space*, and the *fluid*, or *whatever it is*, rushes forward; the *absorbent orifice* now *dilating*, forms a *vacuum*, the *fluid* must therefore *rush in*, and *stimulating it a second time*, obliges it to *contract*. This *contraction* not only *propels* what has *entered the absorbent*, but makes room for a fresh quantity to come forward, and in this way, *perhaps*, is the
matter

matter to be absorbed *taken up* from surfaces.

I cannot be persuaded to admit in general *the principles of inanimate action* into the *theory of living action*. Mr. Hunter thinks, the *bones* are evidently constructed and adapted to one another on *mechanical principles*, that the *same principles* have been consulted, in the *shape, disposition of the fibres*, and *action of muscles*; but that they take place *no where else* in the machine: yet as I find *respiration* is carried on, in part, through the *medium of a vacuum*, I think it also *probable that this principle* may have *some place* in the *process of absorption*.

After substances have once entered the *absorbent vessels*, they are *carried forward* in the same way as the fluid mounts in the *oesophagus* of a *horse* drinking water, or as *liquid fæces* are whirled through the *intestines* in *diarrhœa*: I mean, by the *peristaltic motion* or *mus-*

cular contraction of the different tubes. The *arteries* themselves, independant of the *action* of the *heart*, have a *similar power* over the *blood*, and by their *contractions* encrease the *velocity* of that *fluid*, and the *force* of the *circulation*.

Haller found that when the *absorbent vessels* turgid with their fluids, had *ceased*, in the dying animal, to *contract*, by touching them with *oil of vitriol* they could be made to *contract afresh*, and propelling their contents, became *instantly invisible*.

V. In endeavouring to prove *absorption* in human bodies, I mentioned many *substances*, which we knew had been *absorbed*.

It may be asked, however, *what are the substances more commonly absorbed, and whether there are not very many substances which cannot be absorbed?*

The

The *chyle* and the *lymph* are the fluids more commonly absorbed. Boerhaave supposes, that during digestion, two pounds of *chyle* are every day carried into the blood.

This fluid is white in quadrupeds, transparent in birds, in quadrupeds tastes salt, sometimes coagulates wholly; sometimes, like the blood, forms itself into a *crassamentum*, and a thin fluid, of the same colour as the *crassamentum*, but which does not coagulate.

We have no way of estimating the quantity of *lymph* which is absorbed. It is that fluid which is secreted by the extremities of the arteries, on all surfaces, except the skin, and into every cell of the cellular membrane. It is meant to keep these surfaces and cells moist, that they may move more easily on one another. It also coagulates when collected from the absorbent vessels, is generally transparent, but receives different tinges according

according to the substances it may be occasionally mixed with. I am not certain that it ever *coagulates on surfaces*, on exposure to air, and it resembles considerably, when collected from the *absorbent vessels*, the *coagulable lymph of the blood*. Boerhaave computes, that in consequence of its *circulating*, a quantity equal to *thirty seven pounds of this fluid* passes through the heart in an hour.

Nuck, from experiments made on himself, found that *twelve ounces of saliva* were secreted into the mouth, in *twenty-four hours*; the greatest part of this is most probably *absorbed* with the *chyle* and carried into the blood.

The *bile* and *urine* are probably never absorbed, unless they are accumulated in great quantity, and are not carried off by the *usual outlets*.

An opinion has almost universally prevailed amongst *physiologists*, that the
semen

semen was absorbed from the *testicle*, and employed to *nourish* the body. *Boerhaave* says, “ indeed it is *surprising* that a *stag*, who has been *castrated*, should not in the same manner *cast his horns*, which is a strong argument that *something* returns again from the *semen* into the *blood*, capable of *thrusting out or elongating the fibres and vessels*, so as to cause the *horns* to *fall off* and *grow up again*.”

That *eunuchs* have no *beards*, has been used as an argument to the same purpose.

Men living in *celibacy*, are reputed to have *better health* and to *live longer* than *married men*; and a *horse castrated*, is said to be *stronger* and to *live longer* than one who has not undergone the *same operation*; the *greater strength and longevity*, has in these instances been imputed to an *absorption of the semen*.

The

The opinion appears to me exceedingly *ill-founded*, from the appearance in the *sweat* and *urine* we find that *healthy bile* absorbed in *jaundice*, from *stricture* of the *ducts* or a *stone* sticking in *them*, is absorbed, *only*, to pass off by another passage than the *intestinal tube*, and appears incapable of being converted to any other purpose than it was *originally* intended for.

If the *semen* is not employed for the purposes intended by *nature*, it must have been perceived that it was *thrown off* *involuntarily*. I do not say, that in obstructions of the *epidydymis*, it may not be absorbed to prevent *inflammation* from *distension*; but I have not found that *such constitutions* have been the *strongest*.

That there is a *strong sympathy* between the *head*, the *throat*, and the *organs of generation* in *male animals*, I am firmly persuaded; and that *removing the organs* may be attended with a *removal* of *those parts* that either are *signals of virility*,
or

or intended as a *defence* against the other *males of the species*, who might attempt to *carry off their female*; but that the absence of the *semen* alone would have *this effect*, I doubt much; or that any *other fluid* than the *chyle* is ever employed to *nourish the body*.

Any thing in which the *brain* and *nerves* are much occupied, *wears out the body*. Grief, love, melancholy, intense study, will do this as well as *venery*; and freedom from any of these may have the opposite effect.

A considerable quantity of *atmospheric air* is swallowed with our *food*, and probably, partly *absorbed*. The *air of emphysema*, I formerly observed *was absorbed*. It is even probable that *atmospheric air* may be *absorbed* by the *surface of the body or lungs*. No *elastic air*, however, is found in any part of the body, except in the *alimentary canal*. From experiments it appears, that *arteries*, or

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veins,

veins, cut out in the living body, turgid with *blood*, thrown into water, and placed in the *receiver* of an *air pump*, did not, on *exhausting the air*, shew the least mark of containing *elastic air*. Similar experiments were tried on the *gall bladder*, and with the same effect.

In a case of *ulceration* of the *lungs*, I once exhibited *an ounce* of the *powder* of *sarsaparilla* daily for some weeks; it appeared to have very good effects on the complaint for which it was given; but the *patient* had not taken it *a week*, when he began to complain *that he made bloody water*, and that *there was much sand in his urine*. He had poured *water* on some of the *powdered sarsaparilla*, and found that it contained *much fine sand*. I told him I could not believe it *possible that the sand could get into the blood*; that his now passing *sand* was certainly *accidental*, and *unconnected* with the *powder*. He continued the *powder*, but the sand did not leave the *urine till the powder*

was

was omitted ; nor could I convince him that the sand did not get into the blood.

It would seem necessary, that every thing capable of being *absorbed* should be *dissolved*, or *mixed*, in *liquids*. Powders rubbed on the *skin*, were there no moisture there, would, generally, not be absorbed. I have seen *calomel* lie under the *prepuce*, *unabsorbed*, for some days.

Poisons, the *effluvia*, or the *liquid matter* of *infectious diseases*, we know, may be absorbed. The *venereal disease*, bite of the *mad dog*, the mode of receiving the *jail fever*, and the *inoculated small-pox*, afford proofs of this.

It will not be necessary to observe, that *caustics*, or *concentrated acids*, whose effects are to *destroy the extremities of the vessels*, or very *coarse powders*, even though applied to the *absorbing surfaces* by *liquids*, cannot be absorbed.

It had been *doubted*, whether the *absorbents* would take up *salts*, or very *stimulating substances*; we find, however, *they certainly do*.

If *corrosive sublimate*, thrown into the *stomach*, with a view to cure the *venereal disease*, was not absorbed by the *lacteals*, how should it produce a *salivation*? The *change* it afterwards undergoes in the body, in becoming *quicksilver*, is most probably produced in the *blood vessels*.

Dr. *Hunter* gives a *remarkable case* of some *workmen*, employed to clean a *mineral water well*. They had for this purpose thrown off their *shoes* and *stockings*, and gone into the *well*. The *salts* in the *water* were *absorbed* by the *absorbents* of the feet, and *purged* them all *violently*. I know this has been attributed to the *coldness* of the well. *Cold* applied to the *feet* may have *this effect*; but it is difficult to suppose, that a number of *workmen*,

workmen, accustomed to such employments, should in this instance be violently purged, merely from the coldness of the well.

When *allum* has been exhibited internally, as an *astringent*, in *hæmorrhages*, with success, it is also difficult to conceive, that it was not absorbed by the *lacteals*; and that it had the effect of constringing the vessels of the uterus, for example, merely by acting on the internal surfaces of the stomach and intestines.

An ingenious gentleman has informed us, that when he bathed living rabbits in a solution of nitre for half an hour, that papers dipt in their blood and dried, afterwards flashed in the flame of a candle, and gave evident proofs of the nitre's having gone into the blood. Though I do not doubt his facts, yet I could not on similar trials discover that the blood contained nitre.

The

The *calces of metals*, may certainly be *absorbed*. The *palsy* of the *arms*, so frequent in *house painters*, shews this; and demonstrates from the effects, that the *white lead*, with its *oily vehicle*, had been *certainly*, though *slowly*, *absorbed* by the surfaces of their *hands*.

Did this *poison* affect the *hands only*, I should be tempted to attribute these effects to *sympathy*, and believe that the *action* of the *lead* was confined to *these surfaces only*. But the *constipation*, and *painful affection* of the *intestines* persuade me, that it is also *absorbed*.

Boerhaave asserted, that the *particles* of the *chyle* were *globular*; that the *orifices* of the *laeteals* were only adapted to the *shape* of these *globules*; and that in consequence of this, *no particles* of any other *shape* could be admitted into the *orifices* of the *laeteals*; that though the *particles* might be of the *same shape*, yet if they were *acrimonious*, they *stimulated* the

the *orifice* of the *lacteal*, made it *contract*, or *shut itself up*, and were also in this way *excluded*.

This, *be* considered as a very *fortunate circumstance*, as it prevented any thing *noxious* from getting into the *blood*.

The truth however is, that *very stimulating substances may be absorbed*.

Few things are more *stimulating* to an *irritable surface* than *oil of turpentine*; yet we find, that the *absorbents* of the *skin* certainly *take it up*, and that it may afterwards be *smelt* in the *urine*.

There can be little doubt that the *absorbents* take up the *particles* of powdered *cantbarides* from a *blister plaister*. The *stranguary*, which so frequently takes place, after the application of such a *plaister* to the *head*, sufficiently *proves* this, and shews, that *the same particles*, accumulated in considerable quantity in
the

the *urine*, after having produced *one* inflammation on the *skin*, have been able to produce *a second* in the *bladder*.

However *irritating* the *effluvia* of *garlick* are to the *eyes*, its *juice*, rubbed on the *foot*, may afterwards be *tasted* in the *mouth*.

The *astringency* of some *vegetable decoctions*, does not prevent their being *absorbed*. Dr. *Alexander* informs us, that he cured an *intermittent fever* by *batheing* his *patient's legs* in a *strong decoction* of the *peruvian bark*.

VI. Our next enquiry shall be, *how soon* after they have been applied to the *absorbing surfaces*, will substances, capable of entering the body, generally be *absorbed*; and with what *velocity* do they move *forwards* after they have been *absorbed*?

That some substances may remain on surfaces a very long time before they

they are absorbed, whilst others may be absorbed *almost immediately*, we have numberless examples.

That the *surface* is *naturally* a good or bad absorbing surface; that it is at *that period* disposed or not disposed to absorb; and that the *substance* itself to be absorbed, gives *too much* or *too little* or the *proper stimulus* to the absorbing orifice, are circumstances which will induce *considerable variety* respecting the *time* in which *substances* may be absorbed.

Glysters seldom support a *patient* if he has no other sustenance above *fourteen days*, not only because in the *rectum* and *colon* they are not *sufficiently animalized*, have not received that *change* which the *stomach*, the *mixture* of the *gastric juice*, the *saliva*, the *bile*, and *pancreatic juice*, produce on our food in converting it into *chyle*, but perhaps also because the *former surfaces* are not so good absorbing surfaces as those of the *jejunum* and *ilium*. There are more ab-

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sorbents

forbents in the jejunum than in the ileum, and more in the last and larger ones than on the colon and rectum; a greater absorption is also intended to take place from the former than the latter.*

Mr. Hunter observes, that absorption of poisons seldom takes place from an inflamed surface; a man, for example, is seldom poxed from the inflamed surface of the urethra or glans penis in gonorrhoea; such surfaces, he styles bad absorbing surfaces.

An ulcerated surface, on the contrary, he observes, begins immediately to absorb. No sooner is a chancre visible, than the glands in the groin begin to swell: an ulcer then, in his style, is one of the best surfaces for absorption.

* Though I believe that *glysters* seldom pass the valve of the colon, yet I am convinced, from dissection of the dead body, they sometimes do, and that, assisted by the inverted peristaltic motion of the intestines only, they are, now and then, vomited up, soon after their exhibition.

That

That *surfaces* are sometimes *not disposed to absorb*, appears from observing that it is now and then not possible to produce *ulceration* by the application of a *poisonous or infectious matter*, or to *infect the body*. Many constitutions, without the least precaution, have been exposed to *venereal virus* for years, without being *infected*, and have, however, *been infected at last*.

I inoculated a poor woman and her child; the child caught the *small pox*, the woman, I observed, from the *appearance* in the arm, would not have *them*. I inoculated her a second time; still the little *inflammation* died away on the *third or fourth day*, as it does if one who has had the *small pox* attempts to *inoculate himself* again. I concluded she must have had the *disease formerly*, though in *so slight a way*, as that it had not been observed. She attended her child; and in about *ten or twelve days* after her child was *recovered*, she was taken ill, had a

very great quantity of *small pox pustles*, and narrowly escaped with her life.

Mercury, I have already observed, will sometimes *lye on a surface*, without being *absorbed*; at least if it is absorbed, it is in *so small a quantity*, as to produce *no visible effect* on the *body* or *change* in the *disease*, for which it was applied, but joined to the *volatile liniment*, or if *friction* is employed, will be *readily absorbed*.

Substances which, by themselves, would prove *too stimulating* to make it possible they should be *absorbed*, may, notwithstanding, be *so managed*, as that *their absorption* may be procured.

Thus *mercury* frequently proves *too stimulating* to the *intestines*, and *irritates* their internal surfaces; but if joined with *opium*, the *excess* of *stimulus* may be removed, and the *medicine absorbed*.

Where

Where *opium* has been attended with disagreeable effects on the body, I have known *mercury* given without it, and yet prevented from stimulating the stomach and intestines, by exhibiting it immediately before or after meals.

I have frequently been obliged to give the *bark* in the same way. It is so apt to oppress the stomach, produce lowness and faintness when given on an empty stomach, in some habits, that the patient is uneasy till the *primæ viæ* have thrown off their load by purging or vomiting. Given in this way, however, it has answered pretty well, and proved that it was not necessary to adhere to that general rule, of exhibiting medicines always on an empty stomach.

The presence of one infectious matter will sometimes prevent the absorption of another. A very curious instance of this, I had last summer, in a child I had inoculated at Parsons Green. On the eighth day

day after I had *inoculated* her, the *measles* appeared, and no change had taken place in the arm where the *variolous matter* had been *inserted*. It appeared quite well, nor could I discover where the *puncture* had been made. The *measles* lasted their *usual period*, about *fourteen days*; and on the beginning of the *fourth week* after she had been *inoculated*, the *puncture* in the arm began to *inflame*, formed a fair *small pox pustle*, and the *usual eruption* followed. The child had them of a very mild kind, though she had been very ill of the *measles*, and *recovered*.

A *similar case* formerly occurred to *Mr. Hunter*. He mentioned it to me as a *proof* that *two infectious diseases* could not act on the body at the *same time*.

It is seldom that a *blister* produces *strangury* till it has been applied *twelve* or *twenty-four hours*. This makes it *probable*, that the *particles* of the *cantharides* are not absorbed for many hours after the

the *blister* has been applied; *perhaps* not till it has remained long enough to induce some degree of *ulceration*.

The *venereal virus* often lyes *six weeks* on a *surface* before it is *absorbed*; and were it not *then* to produce a *chancre*, might not be *absorbed* at all. Mr. Hunter observes, that this *virus* will lye *many months* in the *vagina* of a woman without being *absorbed*, or affecting her in the least, whilst a *proof* of its having been *there*, was, that in that time she had *infested* different men.

Variolous matter lyes commonly *eight*, sometimes *fourteen days* in the *wound* before it is *absorbed*; and the *poison* of the *mad dog*, *six weeks*, *three months*, or longer before it produces *hydrophobia*, or has been carried into the blood.

Blood extravasated into the *cellular membrane*, will sometimes remain *months* *there* before it is *wholly absorbed*; and
extravasated

extravasated under the *nails*, or immediately under the *cuticle*, appears *not to be absorbed at all*.

The *skin* tinged with *gunpowder*, or certain *black juices*, in staining devices, on the *arms* of young people, (a practice common with *sailors*,) retains the device *unaltered through life*.

Mr. Hunter observes, that the *small pox pits* in the skin, which were originally formed during *childhood*, become *larger* in the *adult*.

From both circumstances, it appears *probable*, that unless in a *diseased state*, *skin* is *not changed*; but the *pit* becomes *larger* by the *introduction* of *new matter* blended with the *old*. That *new skin* after the healing of *sores*, is always of an *inferior kind*, neither having the *same appearance* nor *same strength* as *originally formed skin*, makes this still *more probable*. It is possible that the *brain* itself may also be exempted

exempted from the *change* or *renovation* which takes place in *other parts*; and could this be *proved*, might furnish those *physiologists*, who *deny* that there are any *lymphatics* in the *brain*, with a *plausible* reason for their not being *there*.

As there is *great variety* respecting the *time* in which *some substances* will be *absorbed*; I believe there is also *some variety* in the *absorption* even of the *chyle* and *lymph*.

The *absorption* of the *latter*, in a *healthy animal*, I believe, is *pretty constant*, but I have no idea of its being near *Boerhaave's quantity*, and *suspect* that it goes on very slowly.

If the *secretion* on *internal surfaces* and in *cells*, was of the *same nature* with that *vapour* thrown off from the *surface* of the *skin*, a *mere water*, and in the *same proportion*, from the *preceding experiments*, the *absorbents* would have a

great deal to do in re-pumping this fluid and preventing dropsy in every cavity and cell. Although I do not allow with Mr. Hewson, that the fluid of surfaces jellies on exposure to air; and though I believe, that a great part of it is similar to the vapour of the lungs and skin, yet, I know there is something more in its composition. I should suspect that the fluid of surfaces was pretty similar to the liquor pericardii, and that as there was to be greater motion in the heart a greater quantity of the same fluid was provided. The liquor pericardii is partly water, partly coagulating lymph, but so combined with the water, that it does not coagulate, unless heat is applied. If I may judge of the nature of the lymph, from the chyle, I should also believe, that it did not generally wholly coagulate; and that in weak constitutions, the quantity of coagulable matter was in a very small proportion. Whilst, in the stronger habit, like the blood it might be more dense, and contain a very large proportion of the coagulating lymph. That the fluid
of

of *internal surfaces* is not like *that* which *exhales* from the surface of the body, appears, from its *colour* and *consistence*. Were it formed from *mere watery vapour*, I cannot conceive how, in the *heat* of the body, it should ever become a *considerable fluid*. The *internal secretion* into *cavities* more probably resembles *sweat*, where there is not only some part of the *thinner vapour* collected by *attraction*, but a great proportion of a *dense fluid*.

Very little fluid seems necessary to *lubricate* these *internal surfaces* of the body; nor does it appear to me necessary, that this fluid should be *perpetually changed*, in order to prevent *putrefaction*. The *water of ascites*, after having remained *ten years* in the *abdomen*, in all probability unchanged, is *perfectly sweet*. A *dead child* may lie *sixteen years* in the *ovarium* or *abdomen* of its *mother*, without ever becoming *putrid*.

I should imagine then, that the *motion* of the *lymph* through the *absorbents*, was in general rather *slow*. The *lymphatic glands*, through which it must so frequently *pass*, induce me also to believe *this*. A drop of *lymph*, in getting from the *great toe* to the *heart*, has, in my opinion, a *four* or *five* times longer journey to make, than a drop of *venal blood*, setting out at the same instant from the same place; the *windings* and *meanders* the former has to pass through, are *inconceivable*; whether you suppose a *lymphatic gland* to be a *congeries* of *contorted*, *convoluted vessels*, or a *collection* of *cells* communicating with one another, but in a *particular way*.

The *motion* of the *chyle* through the *lacteals*, at *particular periods*, is very *rapid*; but as *this absorption of chyle* is *periodic*, I should also believe, that the *absorption* of the *lymph*, though generally *slow*, was, at that time, also *increased*.

When

When a *poison*, or an uncommonly *stimulating matter*, is *absorbing*, I presume, that the *absorption* is then also *very rapid*. I have seen *blotches* on the *skin*, sometimes very *quickly* succeed the appearance of a *chancre*.

Though the *arteries* and *veins* are always *full*, yet I do not believe the *absorbent vessels* are so. I can conceive them, at times, almost *collapsed*, at least, in some parts of the body. The *absorption* from the *bones* may, perhaps, be *constant*. The *lymph*, in its properties, very much resembles the *coagulable lymph of the blood*, and it is not *improbable*, that *many absorbents*, for some particular purpose, not yet known, may be *pretty constantly absorbing it* from the *internal cavities* of the *arteries*; for if the *absorbents* can take up the *red blood* in the *dilated state* of the *artery*, they, probably, take up the *thinner lymph* in its *ordinary state*; on the contrary, unless when they
are

are *absorbing* from the *atmosphere*, the *absorbents* of the *skin* may be frequently *collapsed*. The *absorbents* are hardly *visible* on the *mesentery*, unless at the time the *chyle* is *absorbing*; whereas the *arteries* and *veins* have *uniformly* the same appearance. The *air vessels* of the *lungs* are as *perfect* before birth as ever *they* are *after*; and their remaining *nine months* *collapsed*, during the *fœtal state*, by no means unfits them for *future respiration*. The *corpus spongiosum*, *glandis penis*, and of the *urethra*, is *nothing* else than a *plexus* of *veins*, which may *occasionally* be *turgid* with *blood*, or be *perfectly* *collapsed*, according to *particular states* of the *mind* and *body* in the animal to which it belongs. The *cutaneous veins* themselves, in the *extremities*, are, in *cold weather*, *almost empty*.

When the *periods* of *absorption* take place, I am convinced, the *velocity* of the *absorbed fluids* through their *vessels*, is *very considerable*.

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The *velocity* of the *blood* through the *aorta* of a healthy man, at rest, supposing his *pulse* to be 75 *strokes* in a minute, has been *calculated* to be about *eight inches* in a *second*. As the causes which contribute to *retard* this *velocity* increase, in proportion to the *distance* from the *heart*; the *velocity* of the *blood* in the *extreme vessels*, is, perhaps, only *one-half* of this. I know of no *physiologist* who has so much as *conjectured* what the *velocity* of the *chyle* may be immediately after *digestion* is finished.

On the *mesentery* of many *quadrupeds* there is *no fat*; the *laëteals* run with the *arteries* and *veins* between *transparent membranes*. I had often seen them *turgid* with *chyle*, in the living animal, but never till lately thought of *ascertaining* the *velocity*, with which the *chyle* was *moving forwards*.

Haller says of the *chyle*, That it moves *quickly*; that the *laëteals* just *turgid* with the

the *fluids* he had *obliged* them to *absorb*, *vanished* from his sight.

Leiberkuhn would lead us to suppose the *velocity* of the *chyle* very considerable, when he *calculates*, that the *absorbents* of the *intestinal tube only*, are capable of throwing into the *blood*, *twenty-five pints of liquid in an hour*.

I had an opportunity lately of attending to this *velocity* in the *chyle* in a dog, who had been opened for some other purpose. The *laeteals* appeared *exceedingly numerous*, and *turgid with chyle*. I laid hold of *one* which appeared *running* distinctly, without sending off any *branches*, for *several inches along the mesentery*. I held it between my thumb and finger, close to the *edge* of the *intestine*. The *chyle* between my finger and the *root* of the *mesentery*, *quickly disappeared*. I let the *interrupted chyle* *succeed*, which *as quickly disappeared*. Repeating *this process* frequently, and
very

very quickly, so that as little of the *velocity* of the *chyle* as possible might be attributed to the *elasticity* of the *coats* of a *preternaturally distended vessel*, I found, that the motion of the *chyle* was then considerably greater than at the rate of four inches in a second.

VII. We have some power over the *circulation* of the *blood*; by *stimulants*, we can increase the action of the *heart* and *arteries*, and *quicken* the *blood's motion*; by *sedatives*, we can *diminish* the action of the *former*, and make the motion of the *latter* slower. As *absorption* appears to be of great importance in our *machine*, it may naturally be asked, if we are capable also of *encreasing*, *diminishing*, or, what may be still more important, of *preventing absorption*?—In some instances I think we certainly are.

Stimulating the heart and arteries, also stimulates the absorbents to greater action.

Vomiting is a general stimulus, and gives a *shock* to the whole machine. In consequence of exhibiting *emetics*, the water of *ascites* has sometimes been removed; and when the *cure* has been effected from the *constitution* itself, *vomiting* has frequently been the method employed by *nature*, for keeping up and encreasing the *absorption*.—*Pus*, formed in an *abscess*, has sometimes been removed by artificial *vomiting*, and the opening of the *abscess* been *thus* made unnecessary.

An ounce of *salt of tartar*, given in a mistake for a *drachm of soluble tartar*, and where *purging* only was intended, in a case where there was a very large accumulation of *synovia* in the joint of the *knee*, brought on severe *vomitings*, which continued for forty-eight hours; and induced such an inflammation of the *stomach*, as had nearly proved fatal. During this period, the *patient*, who was naturally *strong*, kept his bed, and drank
warm

warm water, thin broths, or tea, only, and in constant expectation that the *vomiting* would cease *of itself*, had not sent for the *practitioner* who attended him, and who suspecting nothing of this, had not called. When the *patient* had recovered from the inflammation, and the *vomiting* had ceased, *the swelling in the knee* was found almost entirely *dispersed*. The *subsiding* of the *inflammation* could not *here* be attributed to *rest in bed*; the *patient* had been *confined* above a week, before he took the *emetic*, and the *swelling* was *increasing*, not *diminishing*, when the *vomiting* took place.

Where *ædema* has been removed by *rubbing the legs* with *warm oil*, the success must be attributed to the *friction only*, exciting the *absorbents* to greater action; the *oil* serving no other purpose than preventing the *band* from *chafing* the *skin* of the leg.

Mercurial ointment laid on a surface, would, I believe, frequently, have little or no effect; and strong and constant friction is generally necessary to effect a compleat absorption of the medicine.

Cutting down upon a diseased part, by rousing the living power in the part, to greater and more healthy action, will frequently produce an absorption (or removal) of the part. Thus, Mr. Hunter observes, that cutting upon a venereal node, or even blistering it, will sometimes occasion its being absorbed.

Irritating a part also produces and encreases absorption in that part; the intestines wore out by the irritation of long continued purging, go into ulceration, and portions of their internal surfaces are removed.

Pressure, as well as distension, are causes of absorption in parts. Instances of the last I have already given, in speaking of
absorption

absorption from the bladder and tubuli lactiferi.

Every day we meet with instances of the *former*. When the body becomes very *weak*, and the *patient* is long confined to his *bed*, nothing is more common, than that the *inferior parts* of the body not being able to bear the *weight* of the *superior parts*, go into *ulceration*, or are *absorbed*; as in those *ulcers* which are met with opposite to the *great trochanters* of the *thigh bone*, or the *tuberosities* of the *ischia*.

Mercury seems to have considerable *powers* in producing and encreasing *absorption*, in diseased as well as sound parts. We lately heard of its good effects in *procuring* an *absorption* of *water* from the *brain*, in *hydrocephalus*.

I have seen *it* have very great effects in *rheumatism*. The *rigidity* and *pain* in the *muscles* in that *disease*, are often kept
up,

up, I believe, from *adhesions*, formed between the *fasciculi* of *muscular fibres*; and I suspect that *mercury*, by occasioning an *absorption* of *these*, sets the *fibres* at liberty, as well as that by introducing a *new stimulus* into the body, it tends to *remove the old one*.

Mercury occasions an *absorption* of the *alveolar processes*, as we see in those who have undergone *salivations*, where the *teeth* appear to be *longer* than they were, and sometimes drop out.

I am now employing *mercurial ointment* in a case of *dropsy* of the *abdomen*; the *patient* thinks *she diminishes* in bulk, and says, she has been obliged to take in her *flannel waistcoat*.

Mercurial frictions have sometimes had good effects in dispersing tumours on the *joint of the knee*.

The choice of *proper periods* in which medicines may be applied to surfaces, have

have been said to have considerable effect, in encreasing the *absorption* from these surfaces. We have been informed, that in cases where *mercurial ointment* could not be introduced into the body in the evening, the *practitioner* has succeeded, by changing the *time of application* to the morning. The *absorbent system* may, from the *refreshment* of the preceding night's *sleep*, be then in a *more active state*; and if the body is also more liable to receive *infection* in the morning than in the evening, may then take up more readily *infectious matter* as well as *mercury*.

Practitioners often wish to *diminish* the *absorption of pus* from a *sore*, and believe, that the absorbed matter *induces hectic fever*, falls on the *lungs*, and *destroys* the *patient*. I know of no method of *preventing this absorption*. Its effects may be *prevented*, perhaps, by methods which tend to *strengthen the system* in general. Mr. Hunter thinks too much is

is ascribed to *this absorption* of *pus*; rather believes, that *pus absorbed* does very little harm. We see the *bile absorbed* in *jaundice*, notwithstanding the mischief attributed to *it*, has no other effect on the body than that of producing *languor* or *drowsiness*. The *hectic fever*, or the *pulmonary consumption*, consequent to *bad sores*, Mr. Hunter attributes to the *irritation* and *weakening effects* of the *sore*, and not to the *pus*, as any other *long kept up irritation*, with *confinement*, will produce *the same effect*.

When *venereal matter* is *absorbing* from a surface, *this absorption* may be *diminished* by destroying the surface by *caustic*, and converting it into a *common* instead of a *venereal sore*.

In this way, the *absorption* of *poisons* and *infectious matter*, on their first application, may also be prevented. Destroy a *chancre* by *caustic*, if you do not *prevent a pox*, you will, at any rate, make
less

less mercury necessary to the cure. If the wound, from the bite of a mad dog, is destroyed by caustic, immediately after the accident, no absorption can take place, and the disease will always be prevented.

Mr. Hunter thinks, that if the caustic is applied any time within six weeks after the accident, if the wound, having begun to inflame again, (for it frequently heals at first like a common wound) has not yet gone into ulceration, the patient will be secure.

The small pox, should any circumstance make the parent repent of having inoculated the child, may also be prevented by cutting out the bit of skin infected, or destroying it by caustic, within the three or four (perhaps within six or seven) days after the insertion of the variolous matter.

The natural small pox or measles may be prevented frequently in families, by
L I
washing

washing the *bedsteads* or *cradles* of those who have been *infected* in *caustic alkali*.

The *venereal virus* is *prevented* in this way from producing *gonorrhoea* or *lues venerea* itself; I mean by washing the parts which came in contact with the *poison* in a *diluted solution* of *caustic alkali*.

Those who attend the *Lazarettos*, I am told, *prevent* the *matter* of the *plague* itself from *infecting*, by cutting out their *hair* and *washing* the *surface* of the *body*, with *black soap*; which containing more *alkali* than *finer soaps*, combines more perfectly with the *mucus* of the *skin*, and perhaps with the *animal effluvia* themselves, and makes them capable of being *washed off*.

So much for *absorption in general*. A more full account of this *absorbent system*, with a *particular description* and *elegant engravings* of the *absorbent vessels* and *their glands*, the world may soon expect
from

from *Dr. Hunter* himself. I now proceed to the *absorption of calomel from the inside of the mouth.*

On the absorption of calomel.

AFTER what has been premised on *absorption*, I hope, the *method* by which you propose to convey *mercury* into the *blood*, will be more generally understood.

Still, however, before you can persuade *venereal patients* to receive, or *practitioners* to prescribe, *calomel*, in the way you recommend, they may wish to be informed of *some particulars*. They will naturally ask, *whether it is probable that levigated calomel, applied to the surfaces of the body, will be absorbed?*

Or, admitting that *some surfaces* may absorb it, they may wish to know, *whether the surface of the mouth is not such an irritable surface, that it could not*

bear the application of calomel in your way, of course could not absorb it.

They may wish to know *the nature of that evidence* you bring, in support of the *absorption of calomel from the mouth*, and of its having, *in this way*, cured the *venereal disease*.

It is not improbable, that when satisfactory answers may have been given to these questions, they may still continue to ask you, *what superior advantages appear to be gained, by this absorption, over the common methods of curing the venereal disease?*

Some information respecting these particulars may be collected from the following pages.

Your practice recommends itself to me from several considerations.

I. That

I.

That *calomel*, rubbed on the inside of the mouth, may be absorbed, appears probable from analogy.

The *particles* of the blood seen in the simple microscope, are larger than those of levigated *calomel*, yet, as I observed, I have had many opportunities of seeing the *absorbents* turgid with red blood. The *particles* of quicksilver, in the best prepared *mercurial ointment*, are, in the same microscope, as distinct as the *particles* of the blood; yet we all know they are very readily taken up by the *absorbents* of the skin. In a written communication of Dr. Smith's * to you, I observe, that about twelve or fifteen years ago, a medicinal snuff was advertised, in London, and recommended for the cure of *scorbutic* and *cutaneous diseases*. This was tried by a person who had some complaints

* Dr. Smith of Bridge-street, Blackfriars.

of the same nature; from its use his mouth became sore; a salivation, and other symptoms usually consequent to a course of mercury, ensued. In two other instances, I observe he found the application of mercurius emeticus flavus, as a sternutatory, was attended with the same effects. The white precipitate, applied in form of ointment to the head, with a view to destroy certain animalcules, has been blamed, (with what justice I shall not pretend to say) on account of its having sometimes produced salivation. Some months ago, an officer in the army applied to me, for the cure of a gonorrhoea; the injections I ordered him, seemed to encrease the inflammation; but as I had frequently seen local applications cure gonorrhoea, I suspected the disappointment might be owing to the awkward manner of using the syringe, and ordered him to introduce a small bougee, rolled in five grains of calomel, moistened in saliva, evening and morning, into the urethra, for two or three

three inches. We succeeded better ; but on the *third day*, having got wet, his head swelled, and a salivation ensued. Why should not the *particles* of *levigated calomel* be absorbed from the *inside of the mouth*, as well as those of *divided quicksilver* from the *skin*? or of *mercurius emeticus*, from *Schneider's membrane*? or of the *same calomel* from the *urethra*? If it shall afterwards appear, that your *calomel* is applied to the *orifices* of the *absorbents*, partly in a state of *solution* in the *saliva*, the *probability* of its being absorbed from the mouth will be *still greater*, as the *particles* of the *mercury* may then be conceived to be *smaller* than any *levigation* could possibly make them. The *particles* of *levigated calomel* are not much *coarser*, I presume, than those of its *precipitate* by the *volatile alkali*; but this *precipitate*, we are certain, may be absorbed from the *external surface of the body*; why may not the *calomel* be absorbed from the *inside of the mouth*? Mr. Hunter has frequently directed

directed *calomel* to be rubbed on the skin, along with the *volatile liniment*; and seen it have the effects of *mercurial ointment*. The *volatile alkali* in the *liniment*, it is true, decomposes the *calomel*; but still the *precipitate* is a *black mercurial powder*, which, it appears, may be absorbed, and which acts as the *calomel* itself would have done. This very powder, I know, is employed in some parts of the *West Indies* as an excellent dressing to *venereal sores*, and has every effect of *mercury* on these sores. It is extremely probable, that, mixed with *saliva*, it might make a good *mercurial ointment*; or might be exhibited internally with as good effects as the *calomel* itself. However this may be, I am well assured, that, in order to avoid the trouble and save the time employed in making the common *mercurial ointment*, it is usual with some surgeons to precipitate *mercury* from the *nitrous acid*, by means of the *volatile alkali*; this *precipitate*, like that from the *calomel*, is also in form of a black

a black powder, which, after it has been repeatedly washed, and dried, is mixed with *bogs-lard*, and forms *their mercurial ointment*. This is said to have the same, nay, better effects, than the merely divided quicksilver. The earth of mercury, in *mercurius calcinatus*, is one of the best preparations of that metal, yet known, for internal use. With some constitutions, however, it is apt to be too powerful, irritating the stomach and intestines; why should not the earth, got by precipitation from an acid menstruum, be equally efficacious, at the same time that it might, perhaps, be milder in its operation?

My friend, Mr. Smith, informs me, that since the first publication of this letter he has affected the mouth, and cured the first stage of *lues venerea*, by rubbing calomel, mixed with the white ointment, on the thighs.

I have often thought, that too little had been done, by men of real abilities,
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in the way of ascertaining the *absolute effects* of the *different preparations of mercury*. I do not mean that *surgeons* should *try improbable experiments* with their *patients*, or put their safety on a *precarious footing*, when they could put it on an *absolutely certain one*; but cases do happen where *experiments* might be made with *perfect safety* to the *patient*; or the *surgeon*, having first cautiously tried the *effect* of a *preparation* on himself, might *afterwards*, finding that it answered his *expectations*, recommend it, in the *fullest confidence*, to his *patient*. I am led to *this reflection* from finding some *practitioners* placing the *greatest confidence* in two of the *precipitates of mercury* just mentioned, whilst others affirm, they are *absolutely inefficacious*.

II.

The surface, on which you propose the calomel shall be rubbed, is, in its own nature,

ture, a better absorbing surface than, perhaps, any other accessible to friction in the body.

I have endeavoured to *prove*, indeed it is now generally allowed, *that every surface in the body, every cell, absorbs; but it appears also, that the absorbents are by no means equally active, and that, like arteries and veins, they too, from the application of stronger stimuli, may be excited to greater activity. The stimulus of the chyle and lymph on the orifices and coats of the absorbents, is, I presume, the ordinary cause of absorption; but the stimulus of any substance, capable of being absorbed, may be equally a cause of absorption, nay, may be sometimes, especially if conjoined with another, a more exciting cause than the common one. A porter, for example, is engaged for two or three days in rubbing down quicksilver with hogs-lard in a mortar; he works the pestle, the upper end is every now and then smeared with a little*

of the ointment; he gets *fetid breath* and *sore gums*, (*the ordinary effect of mercurial friction*) which shews that *absorption* from the *extraordinary stimulus* had taken place from one of the *thickest cuticular surfaces*, the *palms of his hands*. Though *absorption* may thus take place from *such a surface*, yet in general the *thinner the cuticular surface is*, the *closer* will the *matter*, to be *absorbed*, be applied to the *mouths and coats* of the *absorbents*, and *stimulating them more readily*, will be *sooner absorbed*. Thus *venereal matter*, applied under the *prepuce* in men, or on the *inside of the labium* in women, gets *sooner into the inguinal glands*, *sooner into the habit*, than if it had been applied to the *outside of either parts*. A *little child*, in her *maid's arms*, received a *kiss* from a *girl of the town*, who accidentally passed by. The *cuticular covering* is remarkably *thin* on the *edge of the lips*, and allowing the *blood* to appear more readily through it, gives them their *greater redness*. A *chancre*, on the *projecting part* of the

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the *under-lip*, was the *consequence* of *this salute*; which, in a few days, made its appearance, and resisted *every application* for a *fortnight* or more. At last it yielded to *mercury*; and thus shewing itself to be *venereal*, recalled the circumstance of the *kiss*, which, but for this, had passed *unnoticed*. Had the *venereal matter* been applied to the *cheek*, *externally*, it is probable, from what we see daily, that it might have lain some time without producing *any effect*, and at last have been *wiped off*; or at least that it would not have produced a *chancre* sooner than in a *fortnight*, or perhaps *six weeks*.

As Mr. Hunter finds, that *venereal ulcers* from the *constitution*, (or *secondary ulcers*) are not *infectious*, cannot communicate the *venereal virus*, the *truth* of *this case* may be *suspected*. I do not pretend to say, *what was the nature* of the *sores* in the *girl's mouth*, or *how she got them*. Mr. Hunter, himself, saw the *child's lip*, said that the *sore* looked more like a *chancre* than any thing else, and its
recovering

recovering gradually from the use of mercury, leaves little room, in my opinion, for supposing that it was not a chancre. If the poison makes its way more quickly, from an inside or thin cuticular surface, why should not its antidote do the same thing? Why should not mercury get sooner into the habit from the inside of the mouth than from the outside of the thigh? That the inside of the mouth is a surface better fitted for absorption than any other within the reach of friction, may possibly be denied by some.

How can a glandular, secreting surface, might they say, a surface constantly pouring out, and which, of course, may be presumed to wash off every thing laid on it, be a good absorbing surface? Whatever force this reasoning may seem to have, when applied to dead surfaces, it must lose its weight with those who reflect, that the surface under consideration is a living one. For if it is a good objection against the fitness for absorbing in a surface, that it is a secreting one; the very same objection

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tion lies against *most surfaces*, and may be urged against the *best absorbing surface of the body*, the *internal surface of the intestines*.

III.

The absorption of calomel from the *inside of the mouth*, in your method appears, from the testimony of many of your patients, undeniable.

For if your patients *rub three grains of calomel every day on the inside of the mouth*, and it does not gripe or purge; and if the *common effect of three grains*, taken into the *stomach*, is, that it *certainly gripes and purges*; then we must conclude that the *three grains*, given in your way, have not gone into the *stomach*, whilst their producing evident effects on the *disease*, for which they were exhibited, shews, that they have *certainly got into the habit*, or, in other words, *that they have been absorbed by* the

the surface to which they were applied. One of your patients informed me, that he saw you weigh eight grains of calomel, that he employed all this quantity, in your way, at once ; and that he went through this process three succeeding mornings without being sick, griped, or purged. On what other supposition, shall we be able to say, why these eight grains of calomel did not purge or gripe, but that having been absorbed from the mouth, they became milder in their operation, in the same way as we know the divided quicksilver becomes milder when absorbed by the skin. Besides, since the publication of your book, an eminent physician, I am informed, has exhibited calomel in your way in a case of elephantiasis. The patient was ordered to spit out, whenever the saliva was so much accumulated, as to tempt her to swallow it. She was soon perfectly cured by this method. It must be uncommon obstinacy, that can make one suspect, that in this instance the calomel was not absorbed by the surface of the mouth.

IV. *If*

IV.

If calomel can be absorbed in your way, it must be the most eligible method, because it is less apt to irritate the stomach and intestines, and, by purging, to destroy its proper effect, than it would be if its first action was to be immediately on these parts.

There are, however, some preparations of mercury which may be taken into the stomach without irritating too much, provided opium is exhibited along with them; but there are many constitutions which will not bear opium, and if mercury cannot be exhibited properly without it, patients possessed of such constitutions must be extremely unfortunate, especially if to the former peculiarity of habit is joined another, viz. an antipathy in the skin to every thing oily. How many, originally vigorous, justly deduce their present weak bowels, and crazy

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constitutions, from the mischief occasioned by the action of mercury on the primæ viæ! Now, whether the calomel is here absorbed from the mouth, or not; supposing it actually goes into the stomach; but that the circumstance of its being given in a liquid form, diffuses its particular stimulus, or that this stimulus is blunted by its ropy vehicle the saliva, and that thus only it becomes milder in its operation; still the fact is, that it really is milder, and of course, as the medicine is allowed to be equally efficacious, this method appears preferable to any other mode of exhibiting mercury internally.

With those who believe that the calomel is actually absorbed from the inside of the mouth, and that it does not pass into the stomach and intestines, there will be still less doubt with respect to the propriety of employing this method rather than that of giving calomel, or indeed any other commonly prescribed preparation of

of mercury, in form of draught, bolus, or pill; for though these preparations taken into the stomach and intestines, may frequently be absorbed from their internal surface as well as from any other, yet, from the greater irritability of these surfaces, the stimulus of the calomel, or of other preparations, will more probably prove too powerful; will produce sickness, griping, and purging, and of course occasion their being hurled out of the body before sufficient time has been allowed for their absorption. In this way the remedy runs a greater risk of being entirely lost, and of producing as little effect on the disease, for which it was exhibited, as the Peruvian bark would do on an intermittent, if, instead of staying in the stomach, it was constantly running off by stool. Or though it should not actually purge, yet, from its particular stimulus, the digestive organs, with whose state the functions of the body are so much connected, are more apt to be thrown into disorder; during which

period, the attempts of Nature to relieve herself against any disease, if not altogether prevented, must, at least, be extremely imperfect. On the contrary, if calomel is rubbed on the inside of the mouth, it is applied to a surface, which happens to be alternately exposed to heat and cold, and to considerable friction in chewing our food and cleaning our teeth; of course to a less irritable surface, and capable even of bearing moderate friction. The calomel will here be mixed with the saliva during the friction, will be diffused over the whole mouth, and absorbed from the inside of the lips, surfaces of the tongue, roof of the mouth and fauces, as well as of the cheeks. Thus, its first effects will not be in the way of stimulus on the *primæ viæ*, but it will be gradually and equally applied to the general system. Dr. Hunter gives a remarkable instance of excessive irritability in the stomach and intestines, and of the great advantage gained by being able to introduce the divided quicksilver into the
system

system from another surface, viz. the surface of the skin, after every attempt to make it be taken up by the former more irritable surfaces had been ineffectual. A gentleman who had a venereal ulcer in his throat, and nodes on his bones, was passing through London in his way to Spain, with a view of obtaining in that warmer climate, and from the Lisbon diet-drink, the cure which he had despaired of in his own country, and from mercury. He had tried mercury, internally, in every form, and in the smallest doses; but it constantly produced severe gripings and bloody stools. Dr. Hunter prevailed on him to delay his intended voyage, and to try the effect of keeping his chamber, and rubbing mercurial ointment on the skin. This, at first, had the same effect as the former trials; produced, even in this way, again the gripings and bloody stools. But, by wrapping him in flannel, and confining him to the more constant and equal warmth of his bed, so as to take off that determination

mination to the intestines, which cold, applied to the surface of the body, is apt to produce, and persisting in the use of gentle mercurial frictions, he was at last perfectly cured. If the divided quick-silver thus became milder in its operation, why should not the calomel be improved from a similar treatment?

The particular stimulus of mercury on the more irritable surfaces of the stomach and intestines, and the effects on the whole body, more immediately consequent to this stimulus, may be prevented then, by avoiding these surfaces, and obliging the mercury to find its way into the system through another set of absorbents. I suspect it is also in this way Art has been able to introduce morbid matter itself into the body, so as to make the effects less hazardous than those which Nature left to herself more frequently produces. The advantage, for example, gained over the natural small-pox, by inoculation, is, perhaps, principally to be deduced from
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this circumstance, that the morbid matter, in the natural small-pox, is applied, in great quantity, in form of vapour, to the mouth, nostrils, lungs, stomach, and intestines; and stimulating these irritable surfaces into great irregularity of action, at the same time that it is absorbed from them, produces, as it were, a double disease: whereas in the inoculated small-pox, the morbid matter is applied in small quantity to a small part of the skin, produces its effects gradually, and falls equally on the whole system. This mode of reasoning does not, I know, correspond with the ideas of the more eminent modern physiologists, who think particular contagions, like particular acids or alkalis, are always the same; and produce different effects only in consequence of the bodies, to which they are applied, happening to be different. They maintain, that the quantity of morbid matter, applied to bodies, signifies nothing; that, supposing the thousandth part of a grain of variolous matter produces a hundred
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small-pox, a pint or a gallon of this matter would produce no more; that, at the end of the *small-pox*, there is a greater quantity of variolous matter in the system than ever, and yet it is then perfectly harmless, and has lost the power of irritating. They have also observed, that patients recover of putrid fevers in hospitals, surrounded by others, in every stage of that disease. I have some suspicions, however, that even particular contagions may have different degrees of virulence, according as they are communicated by one body or another, at one period of the disease or another. I have also my doubts, whether the quantity of morbid matter applied is altogether immaterial. For if, after infection, the symptoms following depended on the nature of the infection, conjoined with the state of the body, in which the infection found the patient, then the strongest men should constantly have the mildest disease; which by no means agrees with daily observation. The plague produces its effects, not according

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to the state of the person it invades, but, as Mr. Hunter observes, it brings every kind of body to the same state on the first attack; symptoms of putrefaction, in all sorts of constitutions, immediately taking place. This is by no means the case in common fevers; but as the plague is allowed to be a fever, it would look as if febrile matter, like an acid, be sometimes more, sometimes less concentrated; and might, of course, produce greater or less effects. If the quantity of infectious matter was of no consequence in giving or receiving a disease, cutting out the swelled gland in the venereal buboe, or destroying the chancre on its first appearance, could do no good, because, though in this way, you prevent future absorption, and diminish the quantity of venereal matter that shall be applied to the system, yet as we cannot doubt, that some part of this matter has already gone into the system; the disease, if the quantity of matter is of no consequence, should be as bad as if the buboe had remained,

and the *chancre* had not been destroyed. Some of the *most eminent modern inoculators* have asserted, that when they *inoculated* with the *transparent fluid*, found in the *pustule* on its *first appearance*, they were always *more certain* of the *effect*, viz. That the patient *would be infected*, than when they employed the *matter of a perfectly matured pustule*. They have also said, that when occasionally the *ripe matter* had been used, and the *infection* took place, still the *small-pox* were *later* in appearing, were *more dangerous*, or seemed *somehow imperfect*. I have no experience of the *former method*, having never employed the *lymph*; but my *observations* so far correspond with *theirs*, that, in the last *ten or twelve* I have *inoculated*, where the *matter* employed was always from the *ripe pustule*, and inserted by the *gentlest puncture*, I saw nothing of the *eruption* till the *twelfth or fourteenth day*, though the disease was *perfectly mild* in all of them, and *two of these children* were *inoculated* in the
third

third month. Poisons in the same animal seem to be more concentrated at one time of the year than at another, and to depend for their greater effects on the greater action of the vessels of the animal producing them. The viper-catchers are more afraid of the bite of these animals in the months of June and July, than at any other period. The bite of the young rattlesnake, is said to be perfectly harmless, whilst that of a full grown one is commonly mortal. If, in the first method of inoculation, when long and deep incisions, instead of slight punctures, were made, and when, of course, a greater quantity of variolous matter was applied; if, in this instance, a greater number of small-pox did not follow, the local effects, at least, were frequently terrible; and ill-conditioned, tedious ulcers too often attacked the incised parts. With respect to the observations, that patients have a greater quantity of variolous matter in them at the end of the small-pox than ever was applied to

the *system* before, and that its effects notwithstanding are *imperceptible*; also, that *some patients* recover of *putrid fever*, in *the midst of putrid vapour*; I have only to reply, that nothing is more evident than *the effect of custom*: *bodies*, in *time*, may be made to live on *poison*; and the *first effects* of almost every application to the body, are *greater* than the *subsequent ones*. That the *variolous matter* should at the *end* of the *small-pox* have no effects on the body, is not more *unaccountable* than that we have not the disease *twice*, or *twenty times* instead of *once*. Some *irritations* having *once* acted on us, *lose the power* of ever *affecting us again*; and *others*, though they *may afterwards* affect us again, yet the constitution must be *altered*, with regard to them, and must have returned to its *former state*, before *infection*, ere it can be *acted on again*. That *all infectious vapour* is at *first* chiefly applied to the *more irritable surfaces* of the *stomach* and *intestines*, and that its *first action* is upon
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them, seems more than probable. The indigestion, want of appetite, loathing of food, and uneasy sensation in the stomach and intestines, preceding fever, may possibly arise from the infectious miasmata acting particularly on those surfaces. There are not wanting eminent practitioners in physic, who, from observation, insist, that nothing tends more to destroy putrid fever, on its first attack, than emptying the intestinal tube. The dysentery, caught by infection, is uniformly treated in this way. In the *West Indies*, it is notorious, that in most fevers, unless the bowels are kept open all the while the fever lasts, the patient is certainly lost; and from this circumstance it is, that the cathartic antimony is there held in such high reputation. The purging sometimes preceding the eruption of the small-pox and measles, also contributes to favour this idea.

V. Your

V.

Your friction of calomel is a less tedious, less laborious process, than the common one of rubbing mercurial ointment.

To be obliged to rub, with their own hands, *half a dram of mercurial ointment*, for *half an hour every night*, is a labour only to be conceived by those who have experienced it. Nothing is more universally complained of. What must it be when *half an ounce*, or even *a whole ounce* is rubbed? The process, in short, frequently *tires the patient* so much, that he gives it over before it is *half performed*; and sometimes omits it, when it ought to have been done. It requires *considerable force*; and, if the *patient* is *very weak*, will, to him, be *labour in vain*. I am persuaded, that patients frequently *fail of being cured* from these very circumstances. Let us suppose, *fifteen grains of mercurial ointment* equal in effect to *one grain of calomel*. This calculation, I presume, will not be thought unfair by those who have

have given *both quantities*, on *different occasions*, to *venereal patients*; and have observed, that they could go on with the *calomel*, for *twelve or fifteen days*, sensibly *gaining on the disease*: whereas, when *the ointment* was employed, it became necessary, long before the end of *this period*, to *increase the dose*, in order to keep up *the first effects*. If a *patient*, then, must either *rub fifteen grains of common mercurial ointment*, or *rub a grain of calomel*, and is allowed, after trying both ways, to choose for himself; there will be little doubt with respect to his *choice*. For though *all surfaces absorb*, and any surface may be *stimulated to absorb more* than it commonly does; yet, in general, *surfaces seem to tire*, (if one might say so) and, like *muscles*, having performed a certain *quantity of work*, *refuse to do more*. At least, we find, by experience, that *mercurial ointment* is taken up more quickly by the *absorbents* from diffusing it over a *large surface*, or by *changing surfaces*. Mercurial

curial ointment, of course, being more bulky, and more viscid, than *calomel* and *saliva*, will require a larger surface, and a longer time, before it can be sufficiently rubbed; or, in the common style, before it can be rubbed in. Now if, according to my calculation, three grains of *calomel* have as great an effect on the *venereal virus* as forty-five grains of *mercurial ointment*, and if it would require half an hour's strong friction to make these forty-five grains be absorbed from the whole inside of the thigh, while three grains of *calomel*, gently rubbed on the inside of the mouth, may be absorbed in half an hour, or though it should not be absorbed in twelve hours; still, if it is attended with no trouble to the patient, who does not perceive the advantage of employing *calomel* rather than *mercurial ointment*.

VI.

Your method also recommends itself as a neater and more convenient one, than the rubbing *mercurial ointment*.

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The *friction* of *mercurial ointment* on the *thighs* or *arms*, is, to many people, one of the most *disagreeable things* in the world. Its *lead*en colour, contrasted with the *skin*, makes it look *dirty*. It has a *particularly offensive smell*, independent of that of the *turpentine*, or *balsam of sulphur*, which may have been employed in *extinguishing the quicksilver*. From the circumstance that *half an hour's friction*, or even a *whole hour's friction*, if ever so well performed, will not commonly effect a *total absorption of the ointment*, the *skin* is generally left a little *smeared* with it, after the *process* is over; as it is *oily*, it does not *evaporate*, or *dry*, readily; and will easily *stick* to whatever it *touches*. Patients are obliged to *sleep* in *flannel drawers*, to prevent the *ointment* from getting *through*, and *daubing the bed-clothes*. They must be frequently changing these *drawers*; constantly *washing the skin*; or, as *washing off the ointment* would *retard* rather than *forward* the *cure*, they must remain *dirty* for *six*

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weeks, or perhaps three months together; and if, by accident, any part of the ointment gets upon their linen, from its colour it is very apt to give the alarm in families, and lead to disagreeable discoveries.

VII.

Your method is better, cæteris paribus, than the common modes of exhibiting mercury internally, in as much as it employs friction.

Thin as the cuticular surface is, to which the calomel is applied, friction seems by no means unnecessary; and I should doubt much (as I have already said) if the allowing the powder to lie on the surface, or the mere keeping it in the mouth for ever so long a time, would be attended with the same success. For though I have no idea that friction forces the calomel into the orifices of the absorbents, yet I am convinced that the sti-
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mulus of the *friction*, joined to that of the *calomel*, makes them *take up the mercury* more certainly, and more quickly. I have observed that it was difficult, on any other principle, to say, *why*, in *drop-sical patients*, frequent *frictions* should sometimes bring about the *absorption* of the *extravasated fluids* in the *cellular membrane* of the *legs*; add to this, that, in *friction*, the matter to be *absorbed* is certainly more *closely* applied to the *orifices* of the *absorbents*. There are many *habits* in which *mercurial ointment* would never *eradicate* the *venereal virus*, were it *laid* on the *skin* instead of being *rubbed* on it.

I have *lately* been informed, that your method, in consequence of the *friction* on the *inside of the cheek*, produced *ulceration*. If there was previously a *scorbutic affection* of the *mouth*, or the *friction* was performed *too roughly*, this is, by no means, impossible. It had *no effects* of that kind on me, nor have I ever seen

any instance of its having had *this effect* on any one else.

I have sometimes found the *cure* of *venereal sores* at a *stand*, in cases where the *friction* had been performed by the hands of *delicate* or *indolent patients*; but, on employing the *servants*, and defending *their hands* with *oil-skin gloves*, the *mercury* has been brought immediately to the *mouth*, and the *cure*, beyond controversy, not only *secured*, but *much accelerated*. May not the want of this sort of *friction* in the *stomach* and *intestines*, and the circumstance of the *mercury's* being obliged to lie on these *surfaces* till it is *absorbed*, render it *probable* that *less* of the *mercury* is there *absorbed*, and, of course, give the *external friction of mercury*, independent of the other considerations, the *superiority* over its *internal use*?

Dr. Hunter suspects, that the *friction* of *mercurial ointment* facilitates its
absorption,

absorption, by dividing it still more minutely, and reducing its particles to such a size, as makes them capable of being absorbed.

VIII.

Your method appears preferable to the rubbing mercurial ointment, in as much as watery fluids may be presumed to be more readily absorbed than oils, have not some of the disagreeable qualities of oils, and that there are some reasons to believe that saliva and mercury make a more efficacious compound than mercury and oil.

The vapour, or exhalation from the external surface of the body, is either mere water, or something mixed with water; so that the body may be said to be surrounded with a watery atmosphere of its own vapours. This may be supposed to repel, in some degree, the introduction of oil into the body, by preventing

venting it from coming in *contact* with the *orifices* of the *absorbents*. But as *saliva* approaches more to the nature of a *watery* than an *oily fluid*, though *both* may be *absorbed*, one may presume that the *saliva* will be *more readily absorbed* than *bogs-lard*. That *saliva* may be *more readily absorbed* by the *skin* than *bogs-lard*, may be granted me; but that *saliva* should be *readily absorbed* by the *same surface* which *secreted* it, (*the inside of the mouth*) may seem *improbable*. Let it be remembered, *however*, that the *absorbents* are *intermingled* with the *arteries*; and that *one set of vessels* frequently *take back* what has been *poured out* by the *other*. Again, *saliva*, (or, where that might seem *indelicate*, the *synovia* of *cows* or *sheep*) will occasionally agree better with the *skin* than *oil*. There are some *skins* which seem to have an *antipathy* to *oil*. A *patient* of mine, from rubbing *half a dram* of the *strong mercurial ointment*, two successive nights, on the *inside* of the *thighs*, had an

erysipelatus

erysipelatous inflammation spreading all over the *abdomen*, over the *parts of generation*, and upper parts of the *thighs*. This was followed by *excoriation* of all these parts, a *leprous-like crust*, and a *thin discharge*, with *puffing* in some parts of the *skin*. The parts seemed to me to be *verging fast to mortification*. I was obliged to desist from the *mercury*; to throw in an *ounce and a half* of the *bark* in *substance daily*; give a *couple of grains of opium every night*; and *dress externally with flour*. In a few days he got well of the *inflammation*. I now exhibited the *mercurius calcinatus internally*, with all the *success* I could have wished. But as he had a *chancre* about the *corona glandis*, and found it troublesome to *uncover it* in order to *dress it*, I was willing to give him as little to do this way as possible; and ventured to introduce a *feather, smeared with mercurial ointment*, under the *prepuce*, as a *dressing for the chancre, twice a day*. After one night the *erysipelas* was about

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to be re-produced; nay, the inflammation had actually begun on the body of the penis. I again desisted from the mercurial ointment, went on with the internal use of mercury, and sarsaparilla; and in a few weeks he was perfectly cured. In some instances, opium exhibited internally, has also produced erysipelas.

Here it does not seem that it was mercury which produced this effect; nor opium, for none had been given; nor friction; for, on the second trial, no friction had been employed. I could place the inflammation only to the account of the oil. It may seem strange to deduce an inflammation from so bland a fluid as oil, and yet here it will be difficult to find another cause. Mr. Payne*, from whom my patient had the mercurial ointment, assures me he never uses turpentine or balsam of sulphur to facilitate the division of the quicksilver in making his mercurial

* Apothecary in Coventry-street, Haymarket.

mercurial ointment, but, at the beginning, employs for that purpose *mutton-suet only*. It is possible the *hogs-lard* in *mercurial ointment* may soon become *rancid*; and that it was not *oil*, but *rancid oil*, which produced *this effect*. *Rancid hogs-lard* undoubtedly *breaks down* the *quicksilver* sooner than *fresh lard*, and may be a *temptation* now and then to *save time and labour*. Here this does not seem to have been the case. *Mutton-suet* was here employed on account of its greater *hardness*, and the *recent lard* added afterwards. Besides, if the effects had been owing to *rancidity in the oil*, of which the *mercurial ointment* was made, I am afraid we should see them oftener. At any rate, if this *oil* is apt to become *rancid*, and in this state is to be introduced into the *blood*, *this very circumstance* will be a sufficient motive, with the *thinking practitioner*, to prefer *saliva*, *synovia*, or any other equally convenient, but more healthy, vehicle*.

* Though mercury, applied to the skin, and passing through its absorbents, appears, generally, to act more mildly on the

Further, Mr. Hunter, from some experiments made on himself, thinks it probable, that every preparation of mercury is dissolved in the human fluids, and converted, not only into a new preparation, but constantly into the same kind of preparation, before it acts on the venereal virus in the system. He takes it for granted, that we cannot taste any thing till it is applied in solution to the tongue: that powdered glass, for example, would give no taste, as perfectly insoluble in almost any fluid; but quicksilver itself, held a considerable time in the mouth, gave at last the brassy taste, and must, of course, have been previously dissolved in the saliva. Corrosive sublimate, calomel, mercurius calcinatus, treated in this way, also gave the same taste.

system; yet, I can also conceive such a constitution of the skin, where this general rule may, from that circumstance, be reversed; and that the stomach and intestines may, now and then, bear the stimulus of the mercury better than the skin. Some recent observations have contributed to confirm this last conjecture.

The

The *human saliva*, probably, dissolves many other *metals* besides *quicksilver*. *Pure copper* leaves a very *nauseous taste* in the *mouth*; and *gold*, applied but for a few minutes to the *tongue*, may be *tasted* long after it has been *spit out*. If there is a probability that the *calomel*, during the *friction*, in your method, is in part dissolved in the *saliva*; it may, on this account, be supposed, not only to be more easily absorbed, but also to be applied to the *system* directly in its most efficacious form.

It is not, perhaps, necessary that *mercury* should be in a state of solution in the *body*, in order that it should destroy the *venereal virus*. I can conceive *mercury*, very minutely divided, stimulating the *system*, and acting on the *venereal virus*, merely from coming in contact with surfaces. *Rhubarb* seems to purge and pass the *kidnies* unchanged, as appears from the *yellow tinge* it still gives the *urine*, or *linen* dipt in it. It is more

Qq 2

probable,

probable, however, that substances, capable of *solution* in the *human fluids*, will, upon the whole, have a greater effect than *those* which are *not*.

I was *anxious* to discover the *state* in which *mercury* existed in the *human fluids*; and *chemically* to demonstrate, if possible, that *saliva* and *serum* actually did or did not dissolve quicksilver, and its different preparations. The probable methods of detecting mercury, I imagined, were the following:

First, If it existed in the *fluids* as merely divided quicksilver, I thought I might find it by examining *these fluids* (particularly when dried and powdered) in the *microscope*, or by exposing *these fluids* in different situations to gold, silver, brass, or tin foil. I particularly depended on gold, which unites immediately with this metal, and becomes white.

Secondly,

Secondly, If the *mercury* existed in these fluids in the form of a salt or corrosion, I had still some expectation of finding it by precipitating it from its *menstruum*, by means of the *volatile* or *fixt alkalis*, which throw down nothing from common serum or saliva, but are known to precipitate mercury from acids. If corrosive sublimate, for example, is dissolved in water, on the addition of *volatile alkali*, the mercury falls to the bottom in form of a white powder, and, on adding the *fixt vegetable alkali*, becomes redish or yellow. If nitre of mercury is dissolved in water, on adding *volatile alkali*, the mercury falls to the bottom in form of a black powder; but, by adding the *fixed vegetable alkali*, falls in form of a white one. If saliva or serum contained mercury in a state of solution, or as a salt, I therefore thought it probable that they might part with the mercury to an alkali, or possibly even to an acid; for the *muriatic acid* will take mercury from the *nitrous*, and fall to the bottom

bottom in water, as a *corrosive sublimate* in form of a *white powder*.

Thirdly, I found by experiment, that I could detect *mercury mingled* with certain substances in the state of *cinnabar*, *calx*, or *corrosion*, by burning the substance containing it on a red hot iron: for in this process the *mercury* parting with its *acid* or *sulphur*, and receiving *phlogiston* from the heated iron, is revived into running *mercury* or *quicksilver*.

The *precipitates of mercury* from the *nitrous acid*, and from *corrosive sublimate* in *calomel* by the *volatile alkali* *, I also found by experiment were already in the state of *divided quicksilver* and *amalgamated gold*, and would as readily be discovered as the *mercurial globules of the ointment*, provided they had undergone no change in the body.

* Though *volatile alkali* does not readily part with its *phlogiston*, and appears therefore not *inflammable*, when exposed to fire, yet in certain situations it is *inflammable*; and here parted with its *phlogiston* to the *calces of mercury*.

I have

I have made a great many experiments on this *curious subject*, but the *result* has not been *such* as to leave it in my power to *conclude any thing positively at present*. The *field* I found more *extensive* than I was at first aware of; the experiments required *great accuracy*, and *frequent repetition*; and a *very small alteration* in the mode of repeating them sometimes led to a *conclusion* very *different* from the former; so that, after some pains, I was still obliged to rest satisfied with *probability*.

I am inclined, however, upon the whole, rather to think with Mr. Hunter, that *the mercury* is in the *human fluids* in the form of a *new salt*; since *gold, &c.* were not *amalgamated* by these fluids, obtained in such a state as made them be presumed to be *fully saturated* (if one might say so) *with mercury*. The *gold* was not even *tinged white* in any one place, either when steeped in these fluids *for hours*, or after they had been

been dried, powdered, and then burnt on red hot iron, and the vapours received through an inverted glass funnel on the surface of that metal*.

The *saliva* of a man who had rubbed in near seven ounces of the strong mercurial ointment, and whose mouth notwithstanding had been but a few days sore, shewed no marks of containing quicksilver. Nor was I more successful in attempting to find it in the blood of a person who had rubbed about six ounces, though he then spit but little, nor had any other secretion been apparently increased. The urine of a person who had used still more of the ointment did not appear to contain divided quicksilver, nor was it sensibly different from other urine.

* In one experiment there appeared some white spots on the guinea, which, on putting it into the fire, disappeared: these were owing, I believe, to mercurial vapours; but as this experiment was not confirmed by succeeding ones, I was afraid I had been deceived, and have left the point to be determined by some future trials.

As the *globules of quicksilver* in the *strong mercurial ointment* were still *visible* in the *microscope*, I saw no reason, supposing the *mercury* taken into the *blood*, and that it *there* underwent *no change*, why it should not be still in *some degree visible* in the *microscope*. I examined the *blood* of the *person* just mentioned as having rubbed *six ounces* of the *strong mercurial ointment*; it was not in the least *dissolved*, but was coagulated into a very *firm* and *large crassamentum*. The *serum* had not the *smallest bluish* or *black taint*; the *globules* of the *blood* under the *microscope* had no *unusual appearance*; nor was there the least *semblance* of any thing *extraneous* in the *liquid blood*.

Having *dried* a quantity of the *crassamentum*, and *powdered* it, I again submitted it to the *microscope*, in hopes, that if the *quicksilver* really existed *there*, from the circumstance of *powdering* the *blood*, the *particles* of the *quicksilver*

might have an opportunity of *running together* perhaps, and of course might become more *visible*; but I found not the *least appearance of quicksilver*.

I shook *quicksilver*, in *saliva*, in the *serum of blood*; allowed it to remain in these *fluids many hours*, and even in a *heat equal to that of the human body*. I passed these *fluids afterwards through filtering paper*; but neither *acid*, nor *alkali*, nor *gold applied*, shewed any marks of the presence of any thing *mercurial in these fluids*.

A guinea put into the mouth of a venereal patient in a high salivation, and kept there *a whole day*, shewed no marks of having touched *quicksilver*.

Dr. Garthshore informs me, that a gentleman, who was taking *corrosive sublimate only*, had those parts of the *flute on which he played*, which were *silver*, indisputably tarnished by *quicksilver*.

In

In whatever state *mercury* exists in the blood, I am convinced that it always ex-
hales from the body, after its action is
 over, in the form of running mercury;
 and that it may not only be decomposed,
 but somehow acquire *phlogiston* in the
 human fluids. I am very much deceived
 if I have not repeatedly seen gold rings
 on the fingers, gold watches, and money
 in the pockets, become white and black
 from corrosive sublimate, calomel, or mer-
curius calcinatus, exhibited by the mouth.
 I would not therefore infer that the
saliva does not dissolve quicksilver: the
 solution of metals in human menstrua,
 may be very different from those in the
 mineral acids.

Now, as neither corrosive sublimate,
 calomel, nor *mercurius calcinatus*, in their
 own form, or mixed with water, have the
 least effect in dissolving gold; if ever they
 acquire this property, it must be by being
 revived into quicksilver. The corrosive
 sublimate must part with its acid, and

receive *phlogiston*, in order to become *quicksilver*; the *calomel* must also be *decomposed*, and, losing its *acid*, must, in like manner, receive *phlogiston* before it can be *revived*; and the *mercurius calcinatus*, without parting with any thing, has only to receive *phlogiston* in order to its being again converted into its original *quicksilver*.

As the *heat* of the *human body*, in a sound state, is seldom above 96° of *Fahrenheit's thermometer*; as *feverish heat itself*, is only 108° , and as *mercury* does not *boil*, or become *entirely vapour*, at a less degree of heat than 600° ; it may seem *impossible* that it should ever *exhale* from the surface of the *human body*. Some of our *ancestors*, and even a few of the *moderns*, are of *this opinion*, though for *other reasons*; and hence the report that *mercury* lies in the body, and has been found in *considerable quantity* after death in the *cavities of the bones* in *venereal patients*. They imagined, per-
haps,

haps, that though the quicksilver was kept divided by the oil, with which it was joined, before it entered the body, yet, on its getting there, the oil became more fluid, and gave the quicksilver globules an opportunity of falling down, re-uniting, and forming larger masses, which by their weight forced their way into the cavities of the bones; and having once placed them there, they knew of no powers in the machine which could remove them.

I have never seen *any mercury* in the bones, nor in any other part, in the *dead bodies of venereal patients*, and am persuaded that *mercury* does not remain any considerable time in the *system*.

Dr. Fordyce I think proves, in his *chemical lectures*, that *all the metals* are, at times, in *form of vapour* in the *bowels of the earth*; he also informed us, that *mercurial globules* had been found *adhering* to the *top of the tube of a barometer*, though it had been fixed *immoveable*.

able to the walls of the house for many years.

If some *ores of cinnabar* are placed in a very moderate degree of *heat*, the *mercurial globules* will *sweat out* and form *considerable drops* on their external surfaces. Though *mercury* requires 600° to make it boil, yet like *water* it may perhaps be wholly converted into *vapour* without ever coming near the *boiling point*, or even in a degree of *heat* little more than *that of the atmosphere*; the *evaporation* indeed will be *slower*, and perhaps require a *considerable time* before it is *sensible*. Again, though *mercury* may require a greater degree of *heat* to convert it into *vapour* when in a *large mass*, yet when the *attraction of its particles for one another* is in some degree *diminished by division*, may it not then become *vapour* in a *smaller degree of heat*?

IX. *Your*

IX.

Your method of rubbing calomel looks as if it would be a more expeditious way of giving the mercurial stimulus to the system, and of eradicating the venereal virus.

One of your patients informed me that he formerly had a *chancre* under the *prepuce*; that you had removed it by making him rub a white powder on the inside of his cheek. The effect of the first friction, he said, was, that his mouth became sore and his breath fetid in about six hours after; that his mouth continued so sore that he could not use the friction again for some days; that he only used it four times, but that it always affected his mouth as at first, (so that rubbing twelve grains took him up a month or more); that the chancre mended from the moment his mouth became sore; and that he got perfectly cured from this quantity, and in this manner only.

This

This is a very striking case, but standing single, proves nothing. One grain of mercury may do more with one man than fifty with another. I have been told of a lady who was salivated by taking a quarter of a grain of calomel only. But if your friction shall generally be found to have this effect, it will go a great way to prove that the affecting the mouth is the chief thing in curing the venereal disease; that the quantity of mercury in the system is of no importance, provided the mouth is affected; in short, that acting immediately on the mouth is the quickest and best method of cure; that there is a kind of connection between the state of the salivary glands and the venereal irritation; and that inflaming these glands to a certain degree, and for a certain period, (like cutting upon a node) will destroy the venereal irritation; that calomel rubbed on or near these glands is the most proper substance for producing this inflammation; and that much mercury is not necessary

to the cure of the first stage of lues venerea. From what I have seen of your practice of late, however, I am rather at present disposed to doubt, that the affection of the salivary glands, by mercury, is any further connected with the venereal virus, than as it shews, as a mercurimeter (if the expression may be allowed me) the degree of mercurial stimulus in the system.

I have seldom found, that patients were perfectly safe from a relapse, who were cured, without some considerable affection of the mouth. I have seen the mercurius calcinatus cure the worst stages of lues venerea, without affecting the mouth, but it is in general a very stimulating medicine, and requires the assistance of opium, a gradual encrease of the dose, and a longer period, to confirm the cure.

The more delicate constitutions, may be cured by a small quantity of mercury, and a smaller degree of stimulus

on the salivary glands ; but stronger constitutions, have appeared to me, to have obtained no real advantage, unless the mouth was made sore.

Though I *formerly* supposed, that the calomel applied to the inside of the mouth, might be absorbed in half an hour, and though I believe, that the friction makes a great part of it be taken up during this period, and at any rate diffuses it more generally over the surface of the mouth ; I am now persuaded that the calomel is not wholly absorbed in twenty-four hours, that there is a constant absorption of this powder, taking place, from the mouth, and that some part is also continually washed off by the saliva, and applied to the stomach. In this way a more constant and general stimulus is kept up, with less irritation, than in any other method.

X.

My experiments, so far as they go, confirm your practice. I have not been a
mere

mere speculist with regard to the absorption of calomel: though I have had but little opportunity of trying your method in curing venereal infection, I have seen enough to incline me to believe that you are right, and I have endeavoured to ascertain the absorption of calomel from the mouth, the great hinge on which your method turns.

That I had never taken *a grain* of any thing *mercurial* in my life, did not unfit me by any means from being a proper subject for any experiment respecting the operation of mercury on the body. I took three grains of calomel (your dose), and taking it up by little and little, at the intervals of five or six minutes, on the tip of my tongue, applied it gradually to the inside of the cheeks, lips, roof of the mouth, gums, and of course to the body of the tongue itself, till I had spent about twenty minutes or half an hour in this sort of friction, using as much force as my tongue was capable of, and taking care to swallow as little as possi-

ble, and particularly not to spit during this process. After the *saliva* had accumulated in some quantity, still I continued to wash the mouth with it, and to detain the remaining calomel there as long as possible.

I am disposed to believe that the increased flow of *saliva*, providing it is not swallowed or spit out, rather tends to make the *calomel* be more certainly absorbed; for I have observed, that when *calomel* had been sprinkled as a dry powder on a chancre under the prepuce, great part of it was still to be found next day from want of sufficient quantity of liquid, as well as of friction, to apply it to the orifices of the absorbents. I used the tongue in preference to the finger, because I found it was less apt to bring a great flow of *saliva* into the mouth, and of course did not oblige me to swallow or spit. After waiting twenty-eight hours, I found not the least inconvenience from the friction; there

there was at first *a glow upon my gums and cheeks, a brassy taste in my mouth,* but never any *gripping, sickness, or purging*; nor did my mouth become sore afterwards.

In a day or two after I took three grains of calomel in a small quantity of conserve of hips as a pill, and swallowed it at once. Six hours after it griped me severely and purged. The rubbing of the calomel on the inside of the mouth, I repeated three different times, nor did I find it had any effects different from the first.

Mr. Wells from America, one of my medical friends, rubbed the calomel on the inside of his mouth, without perceiving more inconvenience than I did.

I think it material that the purging should be avoided, and that the medicine should not thus be lost; to accomplish this, and to secure a total absorption of the calomel from the mouth, it may be applied

applied in *smaller doses* and at *greater intervals*; instead of taking *three grains at once*, might not the patient take *one grain at three different times daily*, making the *intervals as long as possible*. In this way there would certainly be a greater chance of its being all absorbed, very little would get into the *stomach*, at least it would be so small a quantity as would not *stimulate the stomach or intestines*, and would produce no *inconvenience*.

Where the *medicine* purges at first, why may not a few drops of *laudanum* be exhibited, previous to the *friction*, and be repeated occasionally?

The *mercurius calcinatus* used in the same way as the *calomel*, or even rubbed on the thigh with *synovia*, would in all probability have the same effects; both of these preparations however, as the *quick-silver*, from whence they are formed, are very little, if at all soluble in watry men-
strua,

strua, and of course, unless it is believed that they are *soluble in the saliva and blood*, may be thought less proper medicines for *an animal whose fluids are of the watry kind*. *Water* which had stood a long time on *calomel* did not become *black* on adding *volatile alkali*; nor did *mercurius calcinatus* appear in the least *diminished* by having been a great while in *water*.

If *the preparations of mercury*, which are *most soluble in watry fluids*, are likely to prove the *most efficacious*, then *corrosive sublimate and nitre of mercury*, as *salts*, and *perfectly soluble in water*, bid fairest for success.

Corrosive sublimate is one of the most *active preparations of mercury*. *Van Swieten* believed it *the best*; it is *soluble in spirit of wine* as well as in *water*, but swallowed by *the mouth* sometimes produces *the worst of consequences*.

Nitre of mercury, or the compound of *mercury* and the *nitrous acid*, (in its fluid state, said to be *Ward's drop*) is perhaps still *superior* to the former in *efficacy*.

Either of these *in powder* would be *too corrosive* probably to admit of being *rubbed*; I see no reason however why a *solution* of either in *water* might not be *absorbed* from the *mouth*, or even from the *feet* or *hands*; and why *these solutions* should not in this way be *milder* and *more efficacious* than they would be were they applied *directly* to the *stomach* and *intestines*.

It has been objected to your method that it *affects* the *breath*, and will lead as certainly to a *discovery*, as the *blue colour* of the *mercurial ointment* on the *linen*.

It is very true, that it might do so, were not *bad breath* a more common accident than the *other*, and were it not so
easily

easily concealed by chewing something aromatic, or scenting one's handkerchief.

It has also been said, that *your mercury affected the mouth locally, without acting on the system. This is really trifling! Mercury cures inflammations very frequently; the more stimulating salts of mercury, used in an uncommon dose, might inflame, but calomel has no stimulus of this kind, and is perfectly mild. I have known it lie for some weeks under the prepuce, and induce no inflammation.*

Thus, Sir, I have examined, with all the attention and impartiality my present situation would admit of, the merits of your proposed method of rubbing calomel on the internal surface of the mouth. The proposal struck me at first; I thought it more than probable you would succeed; I am now satisfied that you have succeeded. It may be objected to you, that your method is not new; other practitioners have rubbed calomel before you. It is possible they have;

T t they

they have said nothing of it, however, to the world; and their claiming merit to themselves now on that score, can have no other effect on you, than that of confirming your practice. Should more weighty objections be even found to lie in practice against your method, than those I have taken notice of, (for what method has no inconveniency?) you will, at any rate, have the merit of having suggested an ingenious idea; of having done your utmost to be useful in your profession; and (if I may be allowed to presage) of having laid the foundation of some excellent future practice in surgery.

I am, Sir,

with sincere regard and esteem,

your obedient humble servant,

WILLIAM CRUIKSHANK.

P O S T S C R I P T.

TH E preceding Letter, is much longer than I intended; but when I began to turn my thoughts to the subject of *absorption*, I found other things connected with it, crowding upon my mind. By this means, the Letter is enlarged far beyond the limits of my first intention, which, I confess, were not *so fit* as they should have been; for most of the experiments related, were made during the time of printing the former part of the letter, and throughout the whole, I noted down my observations, just as they occurred to me, and in the very language I should at that time have spoken them: by which means, the reader will meet

with many *colloquial inaccuracies*, which have escaped my observation, when thinking intensely on *subjects of difficulty and minuteness*.

It may be some extenuation of these inaccuracies that the greatest part of the foregoing sheets were written at those hours which are commonly devoted to *sleep*, as my time during the day was necessarily engaged at *Windmill Street*.

I have had, therefore, no leisure to correct the Letter as I could wish.

As to the *Italics*, which are every where spread over the work, and give it a *motley appearance*, I have only to say, That the little time I was master of made me suspect that I should not be sufficiently understood. If a man writes well, he will always be understood, without *Italics*; but it was *diffidence*, and not *affectation*, which made me, imperceptibly, fall into a practice, which, on maturer reflection, I now perceive is *absurd*.

Having

Having said thus much in excuse for the manner of the Letter, I am afraid, a few words will be necessary also in defence of the matter; especially to those who think, that no *new opinions* in science should be published, till they have been *long weighed* and *attentively considered*.

To those, who love their own fame and reputation better than they do the science they cultivate, *hasty communications* will appear very wrong; but to those who think only of the improvement of science; and who would be happy, at any time, to give up their most favourite ideas, for new ones better founded, this Letter will, I hope, need less excuse.

The publication of new opinions in any science have unquestionably this good effect:—They establish facts on a firmer basis. For if the new opinions are ill founded, the detection of their errors,

errors, serves to confirm the truth of the old opinions :—If they are well founded, they advance science, and root out error, and it is by these means, that we approximate to perfection.

I know men of *real merit*, whose *former valuable observations*, have in time escaped their own memories, and been for ever lost to the public, from an excess of delicacy in publishing their opinions to the world.

Every respect is undoubtedly due to the public; no man should come before them negligently and unprepared: but if his intentions are the improvement of useful science, the good-natured part of mankind will forgive the want of ornament, where they find information.

In order to convince my readers that I am not too tenacious of my opinions, if, on reviewing them, there appears the least room to doubt, I beg leave to make one remark more.

I have

I have somewhere said in this Letter, that *calcareous earth*, exposed to the action of fire, in becoming *quick lime*, gave over *something* to atmospheric air, which converted it into *fixable air*.

The *chemists*, in all probability, will not allow this; but will say, that calcareous earth, in becoming quick lime, gives over to the atmosphere *fixt air* already formed. I own, that vitriolic acid, and calcareous earth, *effervesce in vacuo*, and yield *fixt air*, without the assistance of atmospheric air: but I also believe, that atmospheric air, by receiving *something* from burning bodies, becomes *fixt air*; and I suspect, that calcareous earth, in becoming quick lime, from the action of fire, not only yields *fixt air*, already formed, and which made a part of its composition, but also gives off *something*, which, united to atmospheric air, makes it *fixt air*.

Atmospheric air passing into the lungs, in inspiration, is returned principally

pally fixt air in expiration. I presume, that it is the same air which last entered the lungs in inspiration, which is immediately after returned in expiration, though altered as to its quality. There is one circumstance, I confess, which might be adduced, to support an opinion, that the last inspired air was detained in one set of *air cells* in the lungs, and that already prepared fixt air was returned from another set of *cells* in its stead. The circumstance I allude to is, that the lungs in the dead body (though expiration is the last action of life) always retain more air than is given out at any one expiration.

It is more probable, however, that the same air which was last inspired, is immediately expired, though changed in quality. The air in the bottle, in which I kept my foot an hour, appeared on the trial with lime water to be fixt air, though it did not seem to have acquired any additional bulk during that period.

I suspect

I suspect that it is a particular combination of *phlogiston* and atmospheric air which forms fixt air. The experiment in which the air became fixt by the burning of *phosphorus of urine*, (the idea of which was suggested to me by *Dr. Keir*) seems to prove this.

The *phosphorus of urine* contains *phlogiston*, and a very fixed acid. In burning, it therefore gives over the *purest phlogiston* to the atmosphere. As *phlogiston* joined to atmospheric air produces the same effect on lime water as fixt air, I am led to suspect that fixt air, however obtained, is a combination of atmospheric air and *phlogiston*, or of *something*, in some respects, agreeing with *phlogiston*. Should this opinion appear to be founded on wrong principles by those whose peculiar profession lead them to *chemical experiments*, I shall very readily give it up.—Every man has a regard for his own opinions; but I hope I have still a greater regard for *truth*.

E R R A T A.

Page 9, line 13, *after your, insert doctrine of the.*—P. 9, l. 20, *for absorb, read absorbs.*—P. 12, l. 21, *for begun, read began.*—P. 16, l. 9, *for Aselius, read Asellius.*—P. 19, l. 6, *for taking, read finding.*—P. 23, l. 12, *for at these times, read in the diseased state at least.*—P. 28, l. 13, *for porii, read pori.*—P. 31, l. 18, *for unisone, read unison.*—P. 31, l. 24, *for unisone, read unison.*—P. 41, l. 3, *dele which.*—P. 42, l. 9, *for is, read are.*—P. 43, l. 19, *for Aselius, read Asellius.*—P. 45, l. 9, *for simular, read similar.*—P. 46, l. 24, *before taught, insert first.*—P. 48, l. 5, *for venticulorum, read ventriculorum.*—P. 52, l. 6, *after demonstrate, insert it.*—P. 53, l. 7, *for I will not say that the cuticle does not terminate, read, I have said the cuticle does terminate.*—P. 56, l. 8, *for mēnsenteric, read mesenteric.*—P. 57, l. 18, *for there, read these.*—P. 65, l. 14, *for Ruysche's, read Ruysch's.*—P. 75, l. 23, *after freedom, insert in the villi.*—P. 90, l. 20, *dele not.*—P. 91, l. 3, *for Pollonica, read Polonica.*—P. 91, l. 15, *for his, read this.*—P. 92, l. 3, *for his, read this.*—P. 99, l. 18, *for Buysch, read Ruysch.*—P. 99, l. 21, *for Mekel, read Meckel.*—P. 103, l. 7, *for he attributing, read attribute.*—P. 107, l. 4, *for metalic, read metallic.*—P. 108, l. 1, *dele most readily.*—P. 108, l. 7, *dele filings.*—P. 111, l. 20, *for would, read could.*—P. 117, l. 19, *for a ten times, read ten times a.*—P. 118, l. 2, *for ballance, read balance.*—P. 122, l. 4, *for warm, read water.*—P. 132, l. 19, *for raises, read rises.*—P. 139, l. 17, *for a horse castrated, read a horse not castrated.*—P. 139, l. 19, *dele not and same.*—P. 145, l. 5, *for allum, read alum.*—P. 147, l. 18, *for stranguary, read strangury.*—P. 149, l. 24, *for ilium, read ileum.*—P. 150, l. 1, *for ilium, read ileum.*—P. 152, l. 1, *for pustles, read pustules.*—P. 152, l. 10, *before wil, insert it.*—P. 154, l. 11, *for pustle, read pustule.*—P. 154, l. 20, *for stranguary, read strangury.*—P. 160, l. 7, *for a four or five times, read four or five times a.*—P. 166, l. 14, *dele a.*

